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CONTENTS.

Abstract of an Account of the Keith Fund, prepared by a Committee appointed by the Council of the Royal Society on 4th February 1842, to draw up "An accurate Account of the Foundation, Awards, and present Condition, of the Keith Fund."

The KEITH PRIZE was instituted by the late ALEXANDER KEITH of Dunnottar, Esquire, and it was announced, on the 18th December 1820, to the Royal Society by the following letter, addressed by the Trustees of Mr KEITH to Sir WALTER SCOTT, the President, and the other Members of the Council:—"GENTLEMEN,—It is, no doubt, already known to you, that the late ALEXANDER KEITH, Esq. of Dunnottar, bequeathed the sum of L.1000 for the purpose of Promoting the Interests of Science in Scotland. Having been appointed Trustees for the management of this Fund, we have endeavoured to appropriate it in the most advantageous manner for the advancement of Science, and we have the satisfaction of stating, that the plan which has been adopted met with the special approbation of Mr KEITH himself, to whom it was communicated previous to his death.

"As the Royal Society of Edinburgh is the principal Scientific Establishment in Scotland, we hereby offer to its President and Council the sum of L.600; the principal of which shall on no account be encroached upon, while the interest shall form a Biennial Prize for the most important discoveries in Science made in any part of the world, but communicated by their author to the Royal Society, and published for the first time in their Transactions.

"With regard to the form in which this prize is to be adjudged, we beg leave to suggest that it may be given in a Gold Medal, not exceeding Fifteen Guineas in value, together with a sum of money, or a piece of plate, bearing the devices and inscriptions on the medal.

"If during any of the biennial periods, commencing from Martinmas 1820, no discoveries of sufficient importance shall be communicated to the Society, the interest of the Fund may be added to the principal, after paying the incidental expenses incurred from the preparation of dies and other causes.

"Leaving all other arrangements to your judgment and discretion, we have only to express the hope that this donation may realize the patriotic views of its founder, and contribute in an eminent degree to advance the honour and interests of our native country. We have the honour to be, Gentlemen, your most obedient humble servants, (Signed) ALEXANDER KEITH, J. KEITH, DAVID BREWSTER.—Edinburgh, 4th December 1840.—To Sir WALTER SCOTT, Baronet, President, and the other Members of the Council of the Royal Society of Edinburgh."

In consequence of certain communications which took place between Mr KEITH's Trustees and the Council in March 1833, the former, on 15th April, ad-

dressed the following letter :—" To the President and Council of the Royal Society of Edinburgh. GENTLEMEN,—As we understand that some difficulty has been experienced in the regular biennial adjudication of the Keith Prize Medal, in consequence of it being limited to important discoveries in Science, we hereby consent and direct, that if, during any biennial period, no discovery of sufficient importance be communicated to the Society, in that case, the Council shall adjudge the Prize to the author of the best communication on a *scientific subject*, which shall have been read in the Society during that period, *and on no account shall omit* to confer the Prize at regularly recurring periods of two years. We have the honour to be, &c. &c. (Signed) J. KEITH, D. BREWSTER."

In the year 1834, a Committee was appointed to consider the following points, and report, viz. :—" Whether the existing regulations of the Keith Prize, do not admit of improvement, in respect,—

" 1*st*, Of the biennial adjudication being obligatory.

" 2*d*, Of the expediency of the indefinite adjudication of Medals to any individual.

" 3*d*, Of the papers which should be eligible for competition."

A Report was made by this Committee, and unanimously adopted by the Council on 12th January 1838; and the following is an extract of that part of the Report which refers to the *first two* points :—" That, in the opinion of the Committee, the conditions of the adjudications of the Keith Prize for future periods may be advantageously amended, by absolving the Council from the necessity of awarding the Prize, when, in any biennial period, no paper shall have been communicated which, in the opinion of the Council, is deserving of this distinction.

" That it appears to the Committee, that although some disadvantage may arise from the Prize continuing to be repeatedly adjudged to the same individual, a still greater disadvantage and injustice would attend an absolute restriction from farther competition of persons to whom the Prize may have been adjudged. The Committee, therefore, beg to suggest that such a restriction should not extend to a longer term than two biennial periods, after the lapse of which, the competition should be again open to any person who may have gained this Medal."

The Trustees of Mr KEITH approved of the above Report, in the following letter, addressed to the Secretary, on the 2d February 1838 :—" SIR,—Having considered the Report of the Committee of the Council of the Royal Society of Edinburgh, regarding some alterations in the conditions of adjudication of the Keith Prize, which Report was unanimously adopted by the Council, we, the trustees of the Keith Fund, approve of the alterations therein recommended, and concur in their adoption for the future. (Signed) D. BREWSTER, J. KEITH, M.D."

On the 4th February 1842, a Committee was appointed to draw up an account of the Keith Fund, and, in March 1844, this Committee, along with the account thus drawn up by them (of which this is an abstract), made a Report to

the Council, which, *inter alia*, contained the following recommendation:—"2*d*, The Committee are of opinion, that, in terms of the original appointment by Mr Keith's Trustees, and in order to prevent any doubt or dispute as to the amount of the Prize, or the mode in which it is to be given, it should be settled that the Prize is to consist of a Gold Medal, not exceeding Fifteen Guineas in value, and that the balance of the dividends, interest, or other annual income, derived from the Keith Fund for the biennial period in question, shall be paid over to the successful candidate, to be by him laid out in plate, or otherwise disposed of as he should think fit, with which the Council shall have no concern. That the Secretary of the Society, when announcing the award to the gainer of the Prize, shall state to him the sum which he is to receive along with the Medal, and for which medal and sum he is forthwith to apply to the Treasurer of the Royal Society; and if he shall not apply within six months after the date of the intimation of the award, he shall forfeit the said sum, but shall be entitled to receive the Medal. That, in order that the Secretary may be aware of the amount of the sum to be so awarded the Treasurer shall, on the 1st of January immediately succeeding the expiry of a biennial period, or within 15 days thereafter, give the Secretary a note of the Annual Income of the Keith Fund for the preceding biennial period, and of the value of the Gold Medal which is to be deducted therefrom. The Committee recommend that the Council should authorize the Secretaries and Treasurer to procure a proper die from which the Gold Medal may be struck in future." This recommendation was approved of and adopted by the Council.

The preceding Minutes of Council, contain all that is to be found in the books of the Royal Society, regarding the rules and conditions on which the Keith Prize is to be awarded, and the Committee will now proceed to state the various awards which have been successively made of this Prize since the Institution.

1st Biennial period,		1828–1829.—*Dr Brewster*, for his Papers on his Discovery of Two New Immiscible Fluids in the Cavities of certain Minerals.
2d	do.	1829–1831.—*Sir David Brewster*, for his Paper on a New Analysis of Solar Light.
3d	do.	1831–1833.—*Thomas Graham*, Esq., for his Discovery of the Law of Diffusion of the Gases.
4th	do.	1833–1835.—*Professor Forbes*, for his Paper on the Refraction and Polarization of Heat.
5th	do.	1835–1837.—*John Scott Russell*, Esq., for his Researches on Hydrodynamics.
6th	do.	1837–1839.—*Mr John Shaw*, for his Experiments on the Development and Growth of the Salmon.
7th	do.	1839–1841.—Not awarded.
8th	do.	1841–1843.—*Professor Forbes*, for his Papers on Glaciers.
9th	do.	1843–1845.—Not awarded.

CONDITIONS OF AWARD

OF THE

KEITH PRIZE.

This Prize, the interest of a sum which now amounts to about L.630 left by the late ALEXANDER KEITH, Esq. of Dunnottar, will be awarded by the President and Council of the Royal Society of Edinburgh, on the following conditions :—

I. The author of the best paper on a scientific subject (preference, being in all cases, given to a paper containing an important discovery in science made in any part of the world), communicated, in the first instance, to the Royal Society during the sessions 1845-6, 1846-7, or any two succeeding sessions, shall be entitled to the biennial interest of the KEITH FUND, accruing in the respective periods.

II. The form of the Prize shall be a Gold Medal, of not more than Fifteen Guineas value. The remainder of the sum shall be given in money, to be spent in Plate or otherwise, at the discretion of the receiver.

III. The award being duly intimated to the receiver of the Prize, he is to apply forthwith to the Treasurer of the Society for payment of it; and, failing to do so within six months of the date of the intimation, he shall forfeit the money, but shall be entitled to receive the Medal.

XI.—*On the Sums of the Digits of Numbers.* *By the Right Reverend* Bishop Terrot.

[Read 2d December 1845.]

The general properties of numbers, considered without reference to the notation in which they are expressed, have been very fully investigated by several of the most distinguished mathematicians. Little attention, however, has been paid to the particular properties resulting from the principle of the modern notation, which is the expression of every number in a series, $a + bn + cn^2$, &c. where a, b, c, are the digits, and n the local value or root of the notation. Having been led to examine some of these results, and to account for them, I am now desirous of laying them before the Society. I do not flatter myself that they possess any great practical importance; but as I have reason to believe that they are *new*, I trust the Society will not think them entirely unworthy of their attention.

If, then, we look at the multiplication table, and examine, in the first place, the multiples of *seven*, we find them—

7, 14, 21, 28, 35, 42, 49, 56, 63, 70, 77, &c.
Sums 7, 5, 3, 1, 8, 6, 4, 2, 9, 7, 5, &c.

If we also take, as above, the ultimate sums of the digits of these multiples, that is to say, the sum of the digits of each if that sum be a single digit, or, if not, the sum of the sum of those digits, till in each case we arrive at a single digit, it appears, that, for the first nine places, these sums range through all the digits of our notation, without any recurrence, and then commence over again in the same sequence as before.

On looking at the adjacent line of the multiples of *six*, we find the case very different. The multiples are,

6, 12, 18, 24, 30, 36, 42, 48, 54, 60, 66, 72, &c.
and their sums 6, 3, 9, 6, 3, 9, 6, 3, 9, &c. &c.

Here only three digits occur in the series of sums, and these repeated over and over in the same order. Farther, we may observe, that what is true of *seven* is true of five, eight, and all numbers which are prime to nine; and that what is observed of the multiples of six, occurs also in the multiples of three, the only other digit which has a common divisor with *nine*.

I began with accounting for these facts; and, proceeding from simple multiples to the consideration of other integer series, such as the series of squares, cubes, &c., the successive powers of a given root, the polygonal and figurate numbers, I found that wherever there is a fixed law of relation between the succes-

sive numbers, there is also a definite sequence and recurrence in the sums of the digits which express them ; and the results of these inquiries, with the requisite demonstrations, I will now, as briefly as possible, lay before the Society.

Prop. I.

If m and n are prime to one another, $a\,m$ cannot equal $b\,n$, unless a and b be equimultiples of n and m respectively. For, if $a\,m=b\,n$, $\frac{m}{n}=\frac{b}{a}$. But by hyp. $\frac{m}{n}$ is a fraction in its lowest terms, therefore $b=p\,m$, and $a=p\,n$.

Prop. II.

If $N=P\,.\,\overline{n-1}+r_m$. n being the local value of the notation, and $P\,.\,\overline{n-1}$ being the greatest multiple of $\overline{n-1}$, which is less than N; then r_m is the ultimate sum of the digits of N.

Let $N=a+b\,n+c\,n^2+d\,n^3$, &c.

$$\frac{N}{n-1}=b+c.\overline{n+1}+d.\overline{n^2+n+1}+\ \text{\&c.}\ \ldots\ldots\ +.\frac{a+b+c,\ \text{\&c.}}{n-1}$$

$$N=p.\overline{n-1}+a+b+c,\ \text{\&c.}=p.\overline{n-1}+r_1$$

Again, let $r_1=q.\overline{n-1}+r_2$, where r_2 is the sum of the digits of r_1, or the second sum of the digits of N.

Then $N=\overline{p+q}.\overline{n-1}+r_2$. Let this operation be continued till r_m becomes a single digit, we have $N=P\,.\,\overline{n-1}+r_m$, where r_m is the ultimate sum of the digits of N.

Ex. In our notation $n=10$, and $\overline{n-1}=9$.

Let $N=567434=\overline{63068}\times 9+2$

here 1st sum $=29$
2d do. $=11$
3d do. $=\ 2$

Cor. If $r\ =\overline{n-1}$, then N is a multiple of $n-1$. And, conversely, if N be a multiple of $\overline{n-1}$, $r_m=n-1$.

Prop. III.

If a be a number prime to $\overline{n-1}$; and p, q be two numbers, whose difference is neither $\overline{n-1}$, nor a multiple of $\overline{n-1}$, then $p\,a$ and $q\,a$ cannot have the same ultimate sum.

If possible let $p\,a=m.\overline{n-1}+r$ and $q\,a=m_1.\overline{n-1}+r$, and let $s=q-p$, then $s\,a=q\,a-p\,a=\overline{m_1-m}+\overline{n-1}$; but by hyp. a is prime to $\overline{n-1}$, and s is neither $\overline{n-1}$, nor a multiple of it; therefore, by Prop. I. $s\,a$ cannot equal $\overline{m_1-m}\times n-1$, and therefore $p\,a$ and $q\,a$ cannot have same ultimate sum.

Prop. IV.

If a have a common divisor with $\overline{n-1}$ as v, then $p\,a$ and $q\,a$ will have the same ultimate sum if $q-p=\frac{n-1}{v}$.

Let $p\,a=\text{P}.\overline{n-1}+r$, therefore $q\,a=p\,a+\frac{n-1}{v}\,a=\text{P}.\overline{n-1}+r+\frac{\overline{n-1}}{v}\cdot a$. But v is a divisor of a; therefore $q\,a=\text{P}.\overline{n-1}+r+b.\overline{n-1}=\text{P}_1.\overline{n-1}+r$, that is, $p\,a$ and $q\,a$ have same ultimate sum.

Prop. V.

If a be a divisor of $\overline{n-1}$, or $\frac{n-1}{a}=v$, then $p\,a$ and $q\,a$ will have the same ultimate sum if $q-p=v$.

Let $p\,a=\text{P}.\overline{n-1}+r,\ q\,a=p\,a+v\,a=\text{P}.\overline{n-1}+r+n-1=(\text{P}+1).\overline{n-1}+r$.

Prop. VI.

If $\text{P}=\text{Q}+\text{R}$. The ultimate sum of P = ultimate sum of (sum Q + sum R).

Let
$$\text{Q}=m.\overline{n-1}+r,\ \text{R}=m_1.\overline{n-1}+r_1$$
$$\text{P}=\text{Q}+\text{R}=\overline{m+m_1}.\overline{n-1}+r+r_1$$

But r and r_1 being single digits, their aggregate is either a single digit, or $\overline{n-1}+$ a single digit. In the former case, the ultimate sum of P = sum of Q + sum of R. In the latter, sum of P = sum (sum of Q + sum of R).

Cor. If R be a multiple of $\overline{n-1}$, or $r_1=n-1$, sum of P = sum of Q.

Prop. VII.

From these propositions it follows, that in any arithmetical series, whose common difference is prime to $\overline{n-1}$, the ultimate sum of any term (the p^{th}) = the ultimate sum of $(p+q.\overline{n-1})^{\text{th}}$; but that no two terms at any other interval can have the same ultimate sum; and hence, that all the terms from the p^{th} to the $(p+n-1)^{\text{th}}$ range, as to their ultimate sums, through all the digits of the scale. For if the p^{th} term $=s.\overline{n-1}+r$, then the $(p+q.\overline{n-1})^{\text{th}}$ term $=s.\overline{n-1}+r+q.\overline{n-1}.b$ $=\overline{s+b\,q}.\overline{n-1}+r$.

Again, let p^{th} term$=a$, $q^{\text{th}}=a+\overline{q-p}.b$; but by Prop. I., since b is now taken prime to $\overline{n-1}$, and $\overline{q-p}$ is neither $\overline{n-1}$, nor a multiple of it, the q^{th} term must have an ultimate sum different from the p^{th}.

Ex. 1. The successive multiples of any number prime to 9, are an arithmetic series whose common difference is that number. Thus, the multiples of 5 are,

	5,	10,	15,	20,	25,	30,	35,	40,	45,	50,	55,	60,	&c.
Sums	5,	1,	6,	2,	7,	3,	8,	4,	9,	5,	1,	6,	&c.

Ex. 2. But let the number whose multiples are taken have a common divisor with 9 $(n-1)$ as 6.

The series is 6, 12, 18, 24, 30, 36, 42, 48, 54, 60.

Sums . . 6, 3, 9, 6, 3, 9, &c.

Where the sums recur at every third term, because 6 and 9 have a common divisor 3, and $\frac{9}{3}=3$, that is $\frac{n-1}{v}=3$. (Prop. V.)

Ex. 3. The recurrence of the sums, according to Prop. V., may be more strikingly illustrated, if we use a notation whose root is 13, and, consequently, $\overline{n-1}=12$. If we express the successive multiples of 6 in this notation, we must adopt three additional characters for 10, 11, 12. Let these be $1_0, 1_1, 1_2$. The successive multiples in this notation are,

$6, 1_2, 15, 11_1, 24, 21_0, 33, 39, 42, 48$, &c.

Sums . . $6, 1_2, 6, 1_2\ 6, 1_2$, &c., where we see the sums recur after two terms, because $n-1=12$, and $\frac{12}{6}=2$.

Prop. VIII.

If n be even, and $\overline{n-1}$ consecutive terms of an arithmetic series be taken, the ultimate sum of the digits of their aggregate is $\overline{n-1}$. But if n be odd, the ultimate sum will be $\overline{n-1}$, or sum $\left(\frac{b.\overline{n-1}.\overline{n-2}}{2}\right)$ according as b, the common difference, is even or odd.

For aggregate of $\overline{n-1}$ terms of arithmetic series $= (2a+\overline{n-2}.b)\,.\,\frac{n-1}{2}$

$= a.\overline{n-1}+\frac{\overline{n-1}.\overline{n-2}.b}{2}$. If n be even $\frac{\overline{n-2}.b}{2}$ is integer, whether b be even or odd. Therefore the latter term is a multiple of $\overline{n-1}$, and, consequently, the whole expression being a multiple of $\overline{n-1}$, has $\overline{n-1}$ for its ultimate sum (Prop. II., Cor.) But if n be odd, $\frac{\overline{n-2}.b}{2}$ is integer only when b is even.

Prop. IX.

If we assume as bases two numbers whose sum is $s.\overline{n-1}$, and take a series of the successive powers of each, then of the two series expressing the sums of digits of successive powers, the even terms are identical, while the odd terms are complemental, that is, their sum is $\overline{n-1}$.

Let $m+m_1=s.\overline{n-1}$, $m_1=s.\overline{n-1}-m$

$m_1^p=(s.\overline{n-1})^p-p\,(s.\overline{n-1})^{p-1}m$ . . . . . . $\pm m^p$ according as p is even or odd. Here every term except the last is a multiple of $s.\overline{n-1}$.

Therefore if p be even, sum of $m_1^p=$ sum of m^p.

But if p be odd, sum of $m_1^p=$ sum of $(Q.\overline{n-1})-$sum of $m^p=\overline{n-1}-$ sum of m^p.

To illustrate this and some of the succeeding propositions, I shall here introduce a table of successive powers of digits prime to 9, with their ultimate sums.

$$\text{Base 2}\left\{\begin{array}{llllllll}\text{Powers} & 2, & 4, & 8, & 16, & 32, & 64, & 128, \&c. \\ \text{Sums} & 2, & 4, & 8, & 7, & 5, & 1, & 2, \&c.\end{array}\right\}\text{Sums recur after 6 terms.}$$

$$\text{Base 4}\left\{\begin{array}{llllll}\text{Powers} & 4, & 16, & 64, & 256, & 1024, \&c. \\ \text{Sums} & 4, & 7, & 1, & 4, & 7, \&c.\end{array}\right\}\text{Sums recur after 3 terms.}$$

$$\text{Base 5}\left\{\begin{array}{llllllll}\text{Powers} & 5, & 25, & 125, & 625, & 3125, & 15625, & 78125, \&c. \\ \text{Sums} & 5, & 7, & 8, & 4, & 2, & 1, & 5, \&c.\end{array}\right\}\text{Sums recur after 6 terms.}$$

$$\text{Base 7}\left\{\begin{array}{llllll}\text{Powers} & 7, & 49, & 343, & 2401, & 16807, \&c. \\ \text{Sums} & 7, & 4, & 1, & 7, & 4, \&c.\end{array}\right\}\text{Sums recur after 3 terms.}$$

$$\text{Base 8}\left\{\begin{array}{lllll}\text{Powers} & 8, & 64, & 512, & 4096, \&c. \\ \text{Sums} & 8, & 1, & 8, & 1, \&c.\end{array}\right\}\text{Sums recur after 2 terms.}$$

In this table, we may observe that in every case the sum of the digits recurs, but at different intervals. Next, if we take two complementary bases, as 5 and 4, we find in the lines expressing the sums, that the first terms are respectively 4 and 5, the 2d terms 7 and 7, the 3d 1 and 8, and so on; as was proved generally in the last proposition. Lastly, we may observe that the digits 3, 6, 9, that is $n-1$, and the digits having a common divisor with $\overline{n-1}$, never occur among the sums. It remains, then, for us to point out the reason of this last mentioned fact, and to discover the principle which fixes the period of recurrence.

Prop. X.

Every power of a number prime to $\overline{n-1}$, must have the sum of its digits also prime to $\overline{n-1}$.

Let m, which is prime to $\overline{n-1}$, be reduced to its prime factors, or let $m=a.b.c$, &c.

Then $m^p = (a^p.b^p.c^p$ &c.) Here m^p has no possible divisors except a, b, c, &c., and by hyp. none of these are divisors of $\overline{n-1}$, therefore m^p is prime to $\overline{n-1}$.

Now let $m^p = q.\overline{n-1} + r$. Here $q\,\overline{n=1}$ contains all the divisors of $\overline{n-1}$. If, therefore, r contains any of those divisors $q.\overline{n-1} + r$, or m^p contains such divi-

sors; but m^p has been proved prime to $\overline{n-1}$; therefore r contains no divisor of $\overline{n-1}$, or is prime to it.

Prop. XI.

To determine the recurrence of ultimate sums in the *series* m, m^2, m^3, &c., m being any single digit.

If we can determine what term will have 1 for its ultimate sum, the problem is solved. For if $m^q = p.\overline{n-1}+1$, $m^{q+1} = p'.\overline{n-1}+m$, or has same sum as first term, $m^{q+2}=p''.\overline{n-1}+m^2$, and so on, or sums recur after q terms.

Every number (m) is of the form $3p$ or $3p \pm 1$.

1. If m be of form $3p$, every power of m after the first is a multiple of 9, and consequently the sum of every power $=9$.

2. If m be of form $3p+1$,

$$m^q = \overline{3p}|^q + q.\overline{3p}|^{q-1} \quad . \quad . \quad . \quad . \quad . \quad + q.\frac{q-1}{2}.\overline{3p}|^2 + q.3p+1.$$

In this expansion, every term is divisible by 9, except the two last, or $m^q=9s+3pq+1$.

Consequently m^q will have 1 for its ultimate sum, if $3pq$ = a multiple of 9; but since (m being one of the digits) p cannot $=3$, or a multiple of 3, q must. If, then, $q=3$, $3pq=9p$, and the sum 1 will recur at every third term.

3. If $m=3p-1$, $m^q=\overline{3p}|^q - q.\overline{3p}|^{q-1}$ . . . . $\mp(3pq-1)$, the sign being $-$ if q be even, $+$ if q be odd.

a. Let q be even. $m^q=9s-3pq+1$; and this, as before, will give the ultimate sum 1, if $3pq$ be a multiple of 9, or pq = a multiple of 3. If p be prime to 3, then q must be an even multiple of 3, as 6, 12, &c., or the ultimate sum 1 recurs at every 6 terms. But if p be 3, or a multiple of 3, the sum will recur at every second term, for in that case q may be any even number.

β. If q be odd, $m^q=9s+3pq-1$, but by hyp. $m^q=9r+1$, $\therefore \overline{r-s}.9=3pq-2$, or $3pq-2$ is a multiple of 3, which is absurd. Therefore the sum 1 can never recur at an odd power, when m is of the form $3p-1$.

If, now, we refer to the table given in Prop. IX., we see that of the bases there employed, 4 and 7 are of the form $3p+1$, and in them the sum 1 recurs at every third term. 2, 5, and 8 are of the form $3p-1$. In 2 and 5, p is prime to *three*, and therefore the sum 1 occurs at 6th term. In 8, $p=3$, and therefore the sum 1 recurs at every even term.

Cor. Hence, if m be not a multiple of 3, *i. e.* if it be prime to 9, m^6 has 1 for its ultimate sum, for 1 must occur at 2d, 3d, or 6th term, and 6 is a multiple of 2 and 3; therefore in any case the 6th power must have 1 for its ultimate sum.

Hence $m^6 = 9r+1$ or $9r$; the former when m is prime to 9, the latter when it has a common divisor with it. This is a form not given by BARLOW.

PROP. XII.

In the series of m^{th} powers of successive integers, beginning from 1, the ultimate sums recur after $\overline{n-1}$ terms.

If m be odd, the ultimate sums of any two terms, whose roots together equal $\overline{n-1}$, are either together equal $n-1$, or are each $\overline{n-1}$.

If m be even, the ultimate sums of such complemental terms are identical.

After what has been proved, the demonstration of these is so easy that it is unnecessary to give it.

Ex.	Series of 2d powers	1,	4,	9,	16,	25,	36,	49,	64,	81,	100,	121.
	Ultimate sums	1,	4,	9,	7,	7,	9,	4,	1,	9,	1,	4.

	Series of 5th powers	1,	32,	243,	1024,	3125,	7776,	16807,	32768,	9^5,	10^5.
	Ultimate sums	1,	5,	9,	7,	2,	9,	4,	8,	9,	1.

Here, in the ultimate sums of the squares, we have 1st and 8th, 2d and 7th, &c. identical. In the ultimate sums of 5th powers, the 1st + 8th = 9, 2d + 7th = 9, and so on.

It is worthy of notice, though rather out of place, that if, in the series of 5th powers, instead of taking the sums, we take the difference between the sums of the odd and even digits, the difference will in every case be 1. This property is proved generally by BARLOW, in his Theory of Numbers, p. 172, in this form that $x^{\frac{m-1}{2}}$, where m is a prime number, is of the form $am \pm 1$.

Ex. To illustrate this, and the property of sixth powers mentioned in the XIth Prop., we shall take the 5th and 6th powers of 5 and 8.

$5^5 = 3125$ therefore $d_1 = (5+1)-(3+2) = 1$.
$5^6 = 15625$ therefore $S_1 = 19$, $S_2 = 10$, $S_3 = 1$.
$8^5 = 32768$ therefore $d_1 = (8+7+3)-(6+2) = 10$, $d_2 = 1$.
$8^6 = 262144$ therefore $S_1 = 19$, $S_2 = 10$, $S_3 = 1$.*

COR. From the property above demonstrated of the sixth powers of numbers prime to 9, it follows, that for every such base the seventh power has its ultimate sum equal to the base; that is, that $a^7 = m.9 + a$. For $a^6 = p.9 + 1$, $\therefore a^7 = pa.9 + a$.

Ex. $5^7 = 78125$, $S_1 = 23$, $S_2 = 5$.
$8^7 = 2097152$, $S_1 = 26$, $S_2 = 8$.

* In these equations, d_1, d_2, &c., express the 1st, 2d, &c., differences between the sums of the odd and even digits; S_1, S_2, &c., express the 1st, 2d, &c., sums of all the digits.

PROP. XIII.—*Of Polygonal Numbers.*

In any series of polygonal numbers, n the root of notation being even, the sum of the digits of the $(s+\overline{n-1})^{\text{th}}$ term = sum of s^{th} term.

For every polygonal number is of the form $\mathrm{P}=\frac{\overline{m-2}.s^2-\overline{m-4}.s}{2}$, where m is the number of the order, and s that of the term.

For s substitute $s+\overline{n-1}$,

$$\mathrm{P}'=\frac{\overline{m-2}\times(s+\overline{n-1})^2-(m-4).(s+n-1)}{2}$$

$$=\frac{\overline{m-2}.s^2-\overline{m-4}.s}{2}+\frac{\overline{m-2}.(2\,s.\overline{n-1}+\overline{n-1}|^2)-\overline{m-4}.\overline{n-1}}{2}$$

$$=\mathrm{P}+\overline{n-1}\,.\,\frac{\overline{m-2}.(2\,s+n-1)-\overline{m-4}}{2}.$$

But n being even, the fractional expression is integer, whether m be even or odd. Therefore P and P′ have same ultimate sum. (Prop. VI. Cor.)

If n be odd, the fractional expression is integer only when m is even.

The same inference might at once be drawn from the consideration, that the s^{th} term of any order of polygonals is the sum of s terms of an arithmetical series.

PROP. XIV.

If, as in our notation, n be even, the s^{th}, $(s+p.\overline{n-1})^{\text{th}}$ and $(p.\overline{n-1}-\overline{s+1})^{\text{th}}$ terms of a triangular series have all the same ultimate sum.

In this case, $m=3$, and s^{th} term $=\frac{s.\overline{s+1}}{2}$

Therefore, $(s+p.\overline{n-1})^{\text{th}}=\frac{2\,s\,p+p^2.\overline{n-1}+p}{2}.\overline{n-1}+\frac{s.\overline{s+1}}{2}$. Here the co-efficient of $\overline{n-1}$ is integer, whether p be odd or even: and therefore sum of $(s+p.\overline{n-1})^{\text{th}}$ term = sum of s^{th}.

Again, $(p.\overline{n-1}-\overline{s+1})^{\text{th}}$ term $=\frac{(p.\overline{n-1}-\overline{s+1}).(p.\overline{n-1}-s)}{2}$

$$=\frac{p^2.\overline{n-1}-\overline{2\,s\,p+p}}{2}.\overline{n-1}+\frac{s.\overline{s+1}}{2},$$

and as the coefficient of $\overline{n-1}$ is again integer, whether p be odd or even. Sum of $(p.\overline{n-1}-\overline{s+1})^{\text{th}}$ = sum of s^{th}.

Ex. The triangular numbers are,—

	1,	3,	6,	10,	15,	21,	28,	36,	45,	55,	66,	78,	&c.
Sums,	1,	3,	6,	1,	6,	3,	1,	9,	9,	1,	3,	6,	&c.

Here we observe that the 1st, 7th, and 10th, have the same sum; so also have 2d, 6th, and 11th, and so on.

But the same series expressed in the tredecimal notation, and continued to 13 terms, is

$$1, 3, 6, 1_0, 12, 18, 22, 21_0, 36, 43, 51, 60, 70$$

Sums, $\quad 1, 3, 6, 1_0, 3, 9, 4, 1_2, 9, 7, 6, 6, 7$

Here the 13th term has a sum, 7, different from the first.

But if we take $p=2$, $\overline{n-1}=12$, and $s=11$,

then $p.\overline{n-1}-\overline{s+1}=24-12=12$; therefore 11th and 12th have same sum.

If $s=10$, $26-11=13$, therefore 10th and 13th have same sum, and so on.

Note.—In the decimal series it may be observed, that not only the 1st, 7th, and 10th terms have one for their ultimate sum, but also the 4th, 13th, &c.

This happens, because in the decimal scale, $4=\frac{n-2}{2}$;

but the $\overline{\frac{n-2}{2}}|^{\text{th}}$ term $=\dfrac{\frac{n-2}{2}\cdot\frac{n}{2}}{2}=\dfrac{n^2-2n}{8}=\dfrac{n.\overline{n-2}}{8}=n$, and, consequently, its ultimate sum is 1.

Prop. XV.

If the general term of any series be $ax^m+bx^{m-1}+cx^{m-2} \;\ldots\ldots\; l$; then evidently, if $x+n-1$ be substituted for x, the result will be the original term + a multiple of $n-1$. Or, as in all the preceding forms, the same ultimate sum will recur after $\overline{n-1}$ terms.

If the general term be quadratic $= ax^2+bx+c$.

$$\begin{aligned} \text{Let } x_1=y-x, \text{ then } a x_1^2 &= ay^2-2axy+ax^2 \\ bx_1 &= by-bx \\ c &= c \end{aligned}$$

Therefore
$$\begin{aligned} ax_1^2+bx_1+c &= ax^2+bx+c-\overline{2ay+2b}.x+\overline{ay+b}.y \\ &= ax^2+bx+c+\overline{ay+b}\times\overline{y-2x}. \end{aligned}$$

Now, let y be assumed such, that $ay+b=p.\overline{n-1}$, then the x^{th} and $x_1{}^{\text{th}}$ terms will have same ultimate sum.

Ex. Let x^2+3x+1 be the general term. Substitute for x successively 0, 1, 2, &c., we have the series,

$$1, 5, 11, 19, 29, 41, 55, 71, 89, 109, \&c.$$

Ultimate sums, $\quad 1, 5, 2, 1, 2, 5, 1, 8, 8, 1,$ &c.

Here $a=1$, $b=3$. If, therefore, $y+3=9$, or $y=6$, the two terms in which the

numbers substituted for x are together equal to *six*, will have the same ultimate sum. Thus, in the above series, the 1st and 7th terms, in which 0 and 6 are respectively substituted for x, have the same sum; so also the 2d and 6th, in which 1 and 5 are substituted, and so on.

Prop. XVI.

In the series whose general term is $m.\overline{m+1} \quad . \quad . \quad . \quad . \quad \overline{m+r-1}$, if $m+n-1$ be substituted for m, the ultimate sum of digits will remain as before.

If to each factor we add a, the term becomes

$$\overline{a+m} \times \overline{a+m+1} \times \overline{a+m+2} \quad . \quad . \quad . \quad \overline{a+m+r-1}$$
$$=a^r+p.a^{r-1}+q.a^{r-2} \quad . \quad . \quad . \quad . \quad +m.\overline{m+1}.\overline{m+2} \quad . \quad . \quad . \quad . \quad \overline{m+r-1},$$

where a is a factor of every term except the last. Let $a=n-1$ then term $\overline{m+n-1}.\overline{m+n}$, &c. $=m.\overline{m+1}$, &c. $+s.\overline{n-1}$, whose ultimate sum = that of $m.\overline{m+1}.\overline{m+2}$, &c.

Taking the same general term, if $m+m_1=n-r$, $m^1=\overline{n-1}-\overline{m+r-1}$.

$$\text{Therefore } m_1.\overline{m_1+1}.\overline{m_1+2} \quad . \quad . \quad . \quad \overline{m^1+r-1}=\overline{n-1}-\overline{m+r-1}$$
$$\times \overline{n-1-m+r-2}$$
$$\times \text{ \&c.}$$
$$\times \overline{n-1-m}$$

In this product, $\overline{n-1}$ will enter as a factor into every term except the last, which is $m \,.\, m+1 \quad . \quad . \quad . \quad \overline{m+r-1}$ with the sign $\pm$ according as r is even or odd.

If r be even, the m^{th} and m_1^{th} terms will have the same ultimate sum; but if r be odd, the sums will be complemental.

All the terms from the $\overline{n-r}^{\text{th}}$ to the $\overline{n-1}^{\text{th}}$ must have $\overline{n-1}$ for their sum; because $\overline{n-1}$ must manifestly be a factor in each of them.

Ex. Let $r=2$. Series is 1.2, 2.3, &c.

=2, 6, 12, 20, 30, 42, 56, 72, 90, 110

Sums are, 2, 6, 3, 2, 3, 6, 2, 9, 9, 2, &c.

Let $r=3$, or series 1.2.3, 2.3.4, &c.

=6, 24, 60, 120, 210, 336, 504, 720, 990

Sums, 6, 6, 6, 3, 3, 3, 9, 9, 9, &c.

In the 1st example, r being even $=2$, $m+m_1=10-2=8$; therefore the 1st and 7th, 2d and 6th sums ought to be identical.

In the 2d, r being odd $=3$, $m+m_1=10-3=7$; therefore the 1st and 6th, 2d and 5th, &c. sums are complemental.

Prop. XVIII.—*Series of Figurate Numbers.*

If the series be m, $\dfrac{m.\overline{m+1}}{1.2}$, $\dfrac{m.\overline{m+1}.\overline{m+2}}{1.2.3}$, &c., where each term is the m^{th}

term of the 1st, 2d, 3d, &c., order of figurates, the whole may be reduced to a common denominator, and represented thus :—

$$\frac{2\,.\,3\,.\,4\,\ldots\ldots\,\overline{m-1}.m}{1\,.\,2\,.\,3\,\ldots\ldots\,\overline{m-1}},\ \frac{3\,.\,4\,.\,5\,\ldots\ldots\,m\,.\,\overline{m+1}}{1\,.\,2\,.\,3\,\ldots\ldots\,\overline{m-1}},\ \&c.,$$

of which the numerators follow the law of the series treated in the last proposition. If, therefore, in the series of figurates, the successive sums be taken, and each multiplied by $1\,.\,2\,.\,3\,\ldots\,\overline{m-1}$, the products will form a series recurring after $\overline{n-1}$ terms.

Ex. $4+\frac{4\,.\,5}{1\,.\,2}+\frac{4\,.\,5\,.\,6}{1\,.\,2\,.\,3}$, &c.

$=4+10+20+35+56+84+120+165+220+286+364+455$

Sums, $=4,\ 1,\ 2,\ 8,\ 2,\ 3,\ 3,\ 3,\ 4,\ 7,\ 4,\ 5,$

Multiplying by $1\,.\,2\,.\,3=6$, the sums of products of sums become

6, 6, 3, 3, 3, 9, 9, 9, 6, | 6, 6, 3.

PROP. XIX.—*Of the Ultimate Difference of Digits.*

It is a well known property of digits, that the remainder, when any number is divided by the root of the scale employed $+1$, is equal to the ultimate remainder of the even digits subtracted from the odd; or, using a notation similar to that we have before employed, that $N=p.\overline{n+1}+d_n$. As, however, d_n must always be $+$, if at any step the sum of the even digits be greater than that of the odd, $\overline{n+1}$, or such a multiple of $\overline{n+1}$ as will make it the greater, must be added to the latter.

From this fundamental proposition, a series of propositions analogous to the preceding may be deduced, relating, not to the sums, but to the differences of the digits. The demonstrations are so similar to those already given, that I shall merely illustrate the matter by examining the succession of differences in the series treated in Prop. XV.

The general term was $a\,x^2+b\,x+c$.

Let x become $x+p.\overline{n+1}$, the term becomes

$$a\,x^2+2\,a\,p\,x.\overline{n+1}+a.p^2.\overline{n+1}|^2+b\,x+b\,p.\overline{n+1}+c=a\,x^2+b\,x+c+q.\overline{n+1}.$$

Hence the remainder, after dividing by $\overline{n+1}$, will be the same in both cases, or the ultimate difference will recur after $\overline{n+1}$ terms.

Next, let $x+x_1=y$, then

$$a\,x_1^2+b\,x_1+c=x^2+b\,x+c+\overline{a\,y+b}.\overline{y-2\,x}.$$

Assume $a\,y+b=n+1$, then the two terms will have the same ultimate difference.

Ex. Take as before for the general term $x^2+3\,x+1$,
here $a=1$, $b=3$, $\overline{n+1}=11$, therefore $y+3=11$, or $y=8$.

If, therefore, the sum of the two numbers substituted for x in the expression x^2+3x+1, be 8, the differences in the two cases will be identical.

Substituting successively 0, 1, 2, &c., we have the series

	0	1	2	3	4	5	6	7	8	9	10	11	12	
	1,	5,	11,	19,	29,	41,	55,	71,	89,	109,	131,	155,	181,	&c.
Differences,	1,	5,	0,	8,	7,	8,	0,	5,	1,	10,	10,	1,	5, &c.,	

where we see, 1*st*, that the differences recur after 11 terms; 2*d*, that if 0 and 8, 1 and 7, 2 and 6, &c., be substituted for x, the differences are identical.

EDINBURGH, *December* 2. 1845.

XII.—*Results of the Makerstoun Observations, No. I. On the relation of the Variations of the Horizontal Intensity of the Earth's Magnetism to the Solar and Lunar Periods. With Two Plates. By* J. A. BROUN, *Esq. Communicated by* Sir T. M. BRISBANE, *Bart.*

(Read January 5. 1846.)

1. THE following communication is intended to be the first of a series, in which I propose to consider the results of observations made at Makerstoun, near Kelso, Roxburghshire, in the Observatory of the President of this Society. These observations, and the tabular results, will be found ultimately in the volumes of Makerstoun Observations, constituting volumes of the Society's Transactions.

2. It has been found convenient to separate the observations of the varying intensity of the earth's magnetism into two parts, namely, its resolved components in the horizontal and vertical planes. I shall treat at present of the variations of the horizontal component. These variations are observed by means of the bifilar magnetometer, an instrument devised by M. GAUSS, and modified by Dr LLOYD, described in the Introduction to the Makerstoun Observations for 1842. It consists simply of a magnetic bar, suspended by two silver wires, the latter being twisted out of a vertical plane, the magnet is forced from the magnetic meridian; the variations of its position afterwards are due to two causes, namely, variations of the horizontal component of the earth's magnetic force, or of the moment of free magnetism of the bar; the former are due to changes of the total force or of its dip, the ordinary variations of the latter are due to temperature; and it is, accordingly, a point of much importance to determine the correction for temperature with accuracy, in order that the simple effect of varying intensity may be obtained. I have pointed out, in a paper read before this Society last session, the imperfections of the method usually adopted for the determination of this correction, and the method which has been adopted for the correction of the Makerstoun Observations. I shall afterwards exhibit an example of the very different results to be deduced, after correcting observations by the two methods (11).

3. The horizontal force varies throughout the solar day, having, in that period, two maxima and two minima. The hours of the principal maximum and minimum were first pointed out by M. HANSTEEN, but I am not aware of the first determinations of the secondary points. In the years 1844 and 1845, observations were made at Makerstoun every hour excepting on Sundays. (See Curve, No. 1, Plate III.) From the means of the whole observations for the year 1844, the principal minimum occurs about 20^m past 10 A.M. (Makerstoun mean solar time is used throughout), or exactly when the sun is on the magnetic meridian of Makerstoun; the force then increases rapidly till between 3 and 4 P.M., when there is a slight inflexion; again it increases with its previous rapidity till about $5\frac{1}{2}$ P.M., when *the*

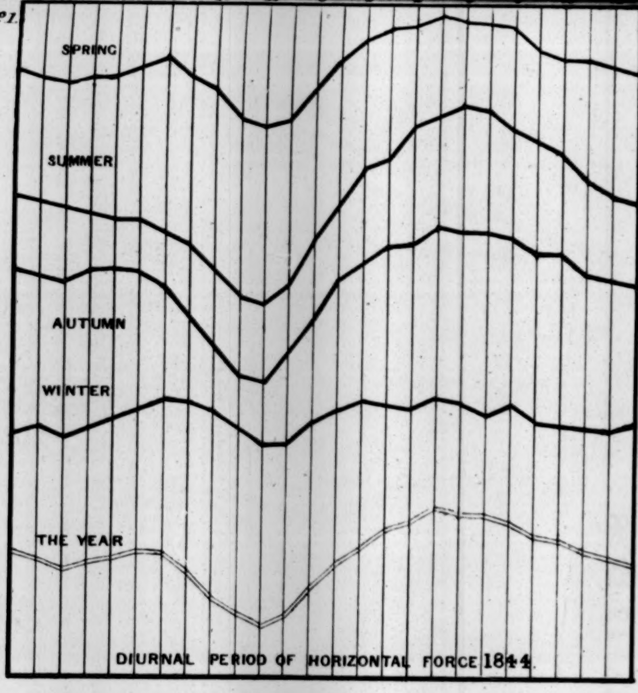
maximum is attained. It now commences to decrease slowly till 8 P.M., more rapidly from 8 till 9, causing another inflexion in the curve, slowly again till 2^h 20^m A.M., when there is *a* minimum; the force then increases slightly till 5^h 30^m or 40^m when *a* maximum occurs, after which it diminishes rapidly till 10^h 20^m A.M., the period of *the* minimum. These hours differ somewhat from the periods obtained at other observatories; and while some part of these differences may be due to errors of temperature correction, I do not think that such errors will altogether account for them, but that the accurate periods of maxima and minima will be found to differ at different places. At Toronto in Canada, for example, *the* maximum occurred a little after 4 P.M. in 1842; and as the mean temperature of the magnet at the succeeding observation hour differs but little from that at 4^h, the period cannot be affected by temperature. Some observatories shew the maximum as late as 7 P.M. It does not, however, seem improbable, that the periods of maxima and minima should differ at different places, when it is known that these periods vary at the same place in the course of the year; at Makerstoun, in 1844, the afternoon maximum occurred as early as 3^h 10^m in December and January, and as late as 6^h 50^m in June; the minimum at 10^h 20^m A.M. in the winter months, and at 9^h 40^m A.M. in June; the A.M. maximum occurs at 6^h 40^m in December, and about 5^h in the summer months, while the earliest minimum occurs nearer midnight in winter than in summer. In this way the periods of the principal maximum and minimum approach to each other, and to noon in *winter*, and remove from each other, and from noon in summer. (See Curve, No. 1.) The reverse to some extent takes place with regard to the periods of the secondary maximum and minimum, which remove from each other in winter, and approach each other in *summer*, till in June the maximum and minimum seem to destroy each other.

The morning maximum is greater than the afternoon one in December;—in November, January, and February, they differ but little from each other; and in December, January, and February, the two minima are nearly equal.

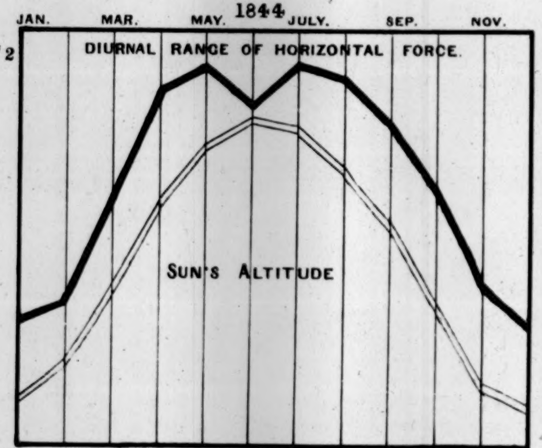
4. The inflexions noted in the mean curve about 3 P.M. and 9 P.M., become minima in the winter months, so that there are then three or four maxima and minima; the smaller ones nearly compensate each other in the mean of the winter months, as they occur at different hours in each month.* I shall consider the cause of these secondary afternoon maxima and minima on another occasion. With regard to the 2 A.M. minimum and 6 A.M. maximum, these seem nearly to vanish in the summer months. In the means for the months of June and July they cannot be detected, excepting that the intensity decreases more rapidly after 6 A.M. than before it; it should not be concluded on this account that this maximum and minimum do not exist. Having projected the hourly observations made in each day of June and July, I have not found one day in ten on which the secondary maxi-

* November, December, and January, have been taken as the three winter months.

No. 1.



No. 2



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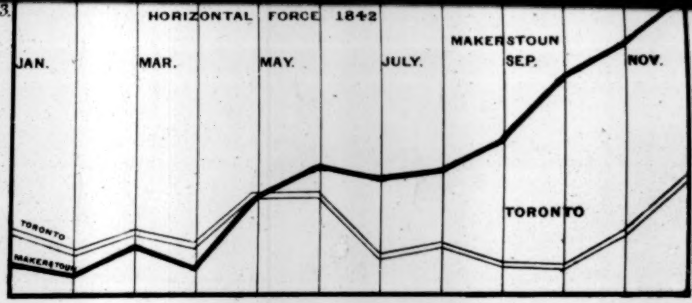
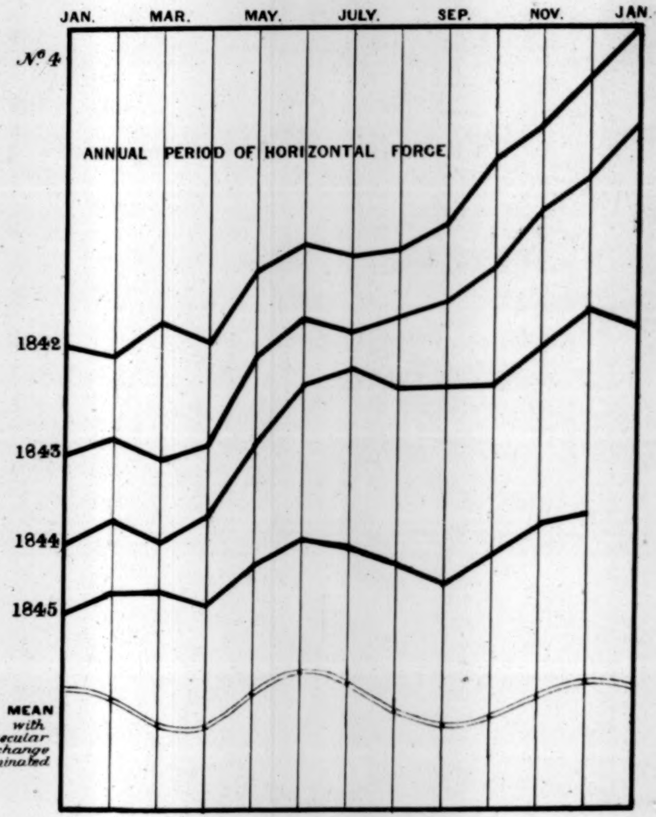


No. 4



mum and minimum do not exist; the periods however, are variable, and in this way they seem to balance each other in the mean; the maximum of one day occurring at the time of the minimum of another. (See Curve, No. 1.) It will be remarked also, with regard to the mean of the summer months, that the descending branch, after the 6 P.M. maximum, has a strongly marked concavity, indicating a tendency to a minimum which does not decidedly shew itself.

5. It has been shewn by Drs LAMONT and LLOYD, that the morning maximum seems to occur throughout the year a little before sunrise, and the afternoon maximum a little before sunset. I would mention another coincidence; the times of maximum atmospheric pressure and minimum intensity are the same throughout the year, and also the times of minimum atmospheric pressure and maximum intensity. There is also a secondary minimum of pressure occurring about the same hour as the secondary maximum of intensity, and the secondary maximum of pressure occurs about 10 P.M., the period of an inflexion in the intensity curve.

6. The diurnal range of intensity is least in January, and greatest in July, being 0·000641 in January (the whole horizontal force being unity), and 0·003396 in July, or five times greater in July than January; the mean diurnal range for the year is 0·002041, being almost exactly the mean of the ranges for January and July, and the same as the ranges for March and October. I have projected the mean range for each month in 1844, and also the mean altitude of the sun for each month. (See Curve, No. 2, Plate III.) There is a considerable similarity in the forms of the two curves; a marked inflexion occurs in June in the range curve. As June is remarkably free from irregularities, it seems to me probable that the range for June is the true diurnal range freed from those irregularities termed disturbances. If so, we may perhaps consider the deviation of the ranges for other months from the curve of altitudes as due to disturbances; in which case we might conclude that disturbances increased the diurnal range most in April and October, and least in February and June. This, however, requires other proof.

7. At Makerstoun, in 1844, each degree of the sun's altitude was equivalent to a diurnal range of about 0·00006 of the horizontal intensity.

8. The mean intensity at midnight and at 1^h P.M. are each equal to the mean of the year, and the means at these hours for each month differ very little from the mean of the month. This leads me to the monthly means, and their relation to the period of a year.

9. In June 1845, I shewed to the Physical Section of the British Association, that the Makerstoun observations of horizontal intensity for 1842 indicated well-marked double maxima and minima in the course of the year; and that as this result had been obtained from the means of only four observations in each day, I had determined the temperature correction approximately for the Toronto bifilar magnet, by the method already referred to, and applied this correction to

the monthly means, as deduced from the observations of horizontal force made at Toronto in 1842, at the following hours:—First, the hours corresponding to the Makerstoun hours; this could not be done exactly, as the observations were made at Toronto every two hours only, and for each of two of the Makerstoun hours, the mean of two of the Toronto hours had to be taken; thus, one of the observation hours at Makerstoun was 7 A.M.; the mean of the Toronto observations at 6 and 8 A.M. was taken as equivalent (see the open curve, No. 3, Plate III.) Second, from the whole two-hourly observations (see the open curve, No. 5, Plate IV.) The monthly means from both these methods, gave the same, or nearly the same, annual period as the Makerstoun observations.

10. I have since then verified this period by the monthly means of the observations made at Makerstoun in the years 1843, 1844, and 1845. I may first refer to the Makerstoun and Toronto curves for 1842, which were exhibited to the Physical Section of the British Association at Cambridge. (See Curves, No. 3, Plate III.) From these I concluded that the horizontal force of the earth's magnetism has an annual period, consisting of a maximum at each solstice, and a minimum at each equinox; both curves present a curious inflexion in March, which I then considered due to some irregularity. The monthly means for the years 1842, 3, 4, and 5, have also been projected together; (see Curves, No. 4, Plate III.) the whole speak the same language, excepting that the inflexion in March 1842 does not occur in the other years, unless it may be said to do so in 1845. All the curves shew a considerable *secular* change, the horizontal intensity increasing throughout the whole period. Whether this be really an increase of the earth's horizontal intensity, or due to some instrumental cause, cannot be positively stated; it is not at all likely to be due to an increase of the free magnetism of the bar, which is suspended at right angles to the magnetic meridian; the only apparent and possible cause is a stretch of the suspension wires; it must be a matter for consideration, whether such a cause is likely to operate for such a period, and to nearly an equal amount for two years. Whether a secular change of horizontal intensity, or due to a stretch of the wires, it is evident that it may be considered as a regular increase throughout the year. Upon this hypothesis I have eliminated this increase from the monthly means of the last three years, and projected the mean below the others. This curve shews more strikingly the annual period of solstitial maxima and equinoctial minima. The minima have nearly the same value; the summer maximum is greater than the winter maximum, but so little, that an error of a thirtieth in the amount of the temperature correction, would account for the difference. The annual range from the mean of the three years is 0·000724, or about the mean diurnal range for the three winter months.

11. M. HANSTEEN concluded from his observations, that there was a maximum of horizontal intensity in December, and a minimum in June. Colonel SABINE con-

cludes from the Toronto observations for 1842 (corrected by the usual method), that there is a maximum in June, and a minimum in December. I have projected the monthly means of the Toronto observations as corrected by the usual method. From these Colonel SABINE draws his conclusion. Under it I have projected the temperature of the magnet in a broken line, and below both, the means from the two-hourly observations, as corrected approximately by myself. (See Curves, No. 5, Plate IV.) These will shew how much depends on the accuracy of the correction in arriving at sound conclusions. I conceive that the consistency of the results at which I have arrived, independently of other considerations, will leave little doubt as to which method of obtaining the corrections should be adopted.

12. It has been already mentioned (10), that the apparent secular change consists of a considerable increase of horizontal intensity. Throughout the whole period, the rapidity of increase has been diminishing, and it is much less in 1845 than in any of the previous years. Of all the puzzling problems in terrestrial magnetism, that of connecting the secular change with some known or observed phenomenon has been the most difficult; any fact, therefore, tending to this, will have interest. One of the first questions which I proposed to myself, connected with it, was whether all hours of the day were equally affected by the secular change? In order to answer this more distinctly, the annual period was eliminated from the monthly means, or, which is nearly the same thing, the mean of each month was reduced to the straight line passing through January and December 1844.* I then found that the mean horizontal force in the first six months of the year 1844, was almost constant one hour *after* the period of the morning maximum, and also that it was almost constant for the last six months, one hour *before* the period of the evening maximum. When the diurnal curve for each month was projected, I found the curves for the first six months to pass through a space of 3-4ths of a scale division in the ordinate of $6^h\ 40^m$ A.M., with the exception of the curve for February, which is very irregular there. The curves for the last six months pass through a space of $1\frac{1}{3}$ scale divisions, in the ordinate of $4^h\ 40^m$ P.M.; the increase of horizontal force from January till December was 18 scale divisions. I next eliminated all the larger disturbances from the monthly means of each hour, but this neither affected the periods of the nodes, nor the values of the ordinates in which they were contained. In this way, then, the horizontal force in its secular progress, seems to rest one foot during the first half of the year about an hour after

* The line should have been drawn through January 1844 and January 1845, but that there is an irregularity in the progress of the horizontal force from December 1844 to January 1845, compared with the previous years. I have, however, also reduced the means to the line passing through January 1844 and January 1845, and find the ordinate of the morning node slightly increased, but that for the evening node diminished.

the morning maximum, and extends the other forward at all other hours of the day, making the greatest strides at the time of the afternoon maximum. During the second half of the year, it rests the previously advancing foot about an hour before the evening maximum, and brings the lagging foot forward at the other hours, but with the greatest rapidity at the time of the principal minimum.

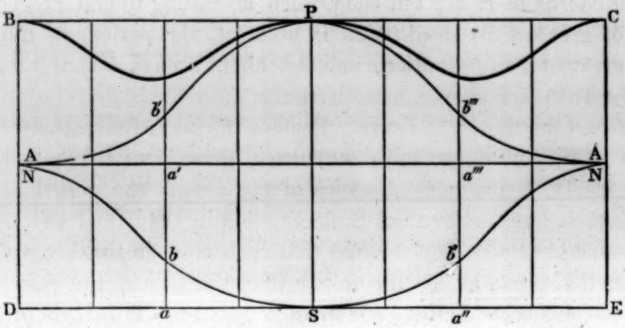
13. There is perhaps nothing more difficult in groping for the laws which regulate certain phenomena than the separation of the effects due to different causes; but it is quite obvious that, before we can arrive at any sound conclusion as to simple laws, this must be done. In the determination of the diurnal period all the observations at each hour for a calendar month or year are summed, and the means taken; in these summations are included several irregularities named disturbances; if the disturbances occurred equally positive and negative at the same hour, or were equally distributed over the twenty-four hours, a large enough series of observations would serve to eliminate them; neither of these suppositions seems to hold, and accordingly, certain hours in some months are more affected by disturbances than the same hour in other months, or than the other hours of the same month; the diurnal curve, therefore, is complex. There are other causes, as will be seen afterwards, which render it more so.

14. In the attempt to determine whether the horizontal intensity varies with the moon's declination, the days were numbered from the day of the moon's greatest N. declination, counting that day 0 till it returned to the greatest N. declination again; and, as 13 of the moon's revolutions, with regard to node, are equivalent to 12 lunations, and nearly to a year, the 13 revolutions, with regard to declination, were selected for summation; as, by this means, any effect due to varying phase, or to annual period, would be eliminated. The mean intensity for each of the 13 days on which the moon had its greatest N. declination were then summed together; the means for the 13 days numbered 1, in which the moon was moving south, and so on. For the purpose of verifying the result thus obtained, similar summations of the observations for 1845 were made; in this case, however, only 12 revolutions, with respect to declination, were obtained, so that any effect of phase will not be perfectly eliminated. No attempt has in either case been made to eliminate disturbances. The results of these summations were projected, having previously eliminated the effect of secular change. (See Curves, No. 6, Plate IV.) The curve, from the observations of 1844, indicates a maximum about 2 days after the moon has attained its greatest S. declination, and a maximum about a day after it has attained its greatest N. declination—the maxima have nearly equal values, so also have the minima. The branches ascending to and descending from the period of greatest S. declination are greatest; so that the periods of minima are nearer, the greatest N. declination being about 5 days before and after it. The curve deduced from the observations of 1845 shews the maxima nearly at

the same periods as in 1844; but the branch ascending to and descending from the period of greatest N. declination is greatest, the periods of minima being nearer the greatest S. declination, namely, about 5 days before it and after it. The curve for 1845 is, however, more irregular after the S. declination maximum than in any part of the other curve. Besides the non-elimination of the effect connected with varying phase and disturbance, there is another possible cause of difference, namely, the varying distance of the moon; the period of perigee is about two days before the greatest S. declination in 1844, and two days after it in 1845. It should also be remembered that each point in these curves is a mean of only 12 or 13 days; as for the minor irregularities in the positions of the points, it is obvious that, as there are 27 days between the periods of the moon's greatest N. declination, if the full moon occurs on the day of greatest N. declination in one month, it will occur on the second day after the greatest N. declination on the next month, the fourth day on the next, and so on. It will be seen afterwards that this will cause a slight irregularity. It is on this account that I have projected the curves among the points, giving a preference to the mean positions of each two points.

15. The similarity of the positions of maxima and minima in these curves, having the moon's declination for abscissæ to the annual curve, or that having the sun's declination for abscissæ, is at once evident; by taking the mean of the two lunar curves, however, the cases will be identical, for then the moon's perigee will occur at the time of its greatest S. declination, and its apogee at the time of the greatest N. declination; this is the case with us for the sun. The resulting means have been projected below the other curves. By comparing the mean curves of No. 4 and No. 6, it is at once obvious that the facts are the same for both the sun and moon. I conceive, then, that I am justified in stating that the same relation exists for the moon as for the sun between the variations of the horizontal component of the earth's magnetic intensity, and the variations of declination and parallax.

16. We have, then, a law connected with two periods, namely, distance and declination. To which does it belong, or does it belong to both? It will take a few years' observations to determine this for the moon: it may be determined for the sun by observations for the annual period made in the Southern Hemisphere. Is there a maximum at the greatest N. declination, and also at the greatest S. declination; or have changes of declination no effect? and are the maxima due to the moon's or sun's distance solely? The supposition that at first sight seems most probable is, that these variations are due to both; that a maximum occurs at the time of perigee, a minimum at the apogee, a maximum at the greatest N. declination, and a minimum at the greatest S. declination. It may easily be shewn that two regular curves having these arguments, when superposed, would



produce two minima. Thus, if A P A be the curve due to distance, N S N that due to declination, the curve B P C, produced by the superposition of their ordinates will have two minima, if the sum of the ordinates $ab + a'b'$ be less than the sum DN + AB, and $a''b'' + a'''b'''$ be less than EN + AC.

The fact, that in both the solar and lunar curves the maxima are nearly equal, is against the supposition that both distance and declination are equally concerned, as it seems rather improbable that the effect of increasing distance should precisely counterbalance the effect of increasing N. declination. We have, however, much more singular cases of compensation in the motions of the heavenly bodies.

17. The range of the lunar declination curve for 1844 is 0·000455; for 1845, 0·000390; and, for the mean of both years, 0·000380.

18. I have already mentioned (14), that, by taking 13 revolutions of the moon, with respect to its declination, we eliminate any effect due to the varying phase of the moon. Similarly, if we take 12 lunations, and sum the mean intensity for the twelve days on which the moon was full, the twelve days on which it was one day old, and so on, we eliminate the effect of varying declination, and also the annual period very nearly. If, however, we may consider the intensity with respect to N. declination similar to that with respect to S. declination, it is evident that 6 lunations will be sufficient to eliminate the effect of declination. (See Curves, No. 7, Plate IV.) I have had the observations during the six summer lunations for 1844 summed by themselves, and also those during the six winter lunations; the mean intensities for both, for each day of the moon's age, have been projected, and also the mean for the year. All indicate a maximum of intensity about two days after the new moon, and a minimum perhaps two days after the full moon; the summer curve has an irregularity before full moon, and its range is only half that for the winter months. The minor irregularities may be accounted for in the same way as for the declination curve. The range for the

winter months is 0·001040. It appears to me, however, that it is exaggerated, owing to the curious fact, that the chief negative disturbances in 1844 occurred about the time of full moon.

19. It has not appeared to me necessary to verify this law by the result of another year's observations. Each of the winter months of 1844 shews the facts as completely as the mean; in the summer months, the result is not so evident. It would appear as if the effect of phase swallowed up the effect of declination in the winter, while the reverse occurred in summer. I have projected the means of the horizontal intensity for each day from January 4th till April 3d, 1844, including three synodical periods. (See Curves, No. 8, Plate IV.) In each period the curve shews the facts most completely; and the lunations in September, October, November, and December, shew them perhaps better. The periods of greatest N. and S. declination, and of the syzygies, are indicated on the curves, the open O being full moon. There are several curious facts, in connexion with the observations projected, which I cannot enter fully into at present; I may remark, however, the appearance of a weekly period. No observations being made on Sundays, breaks occur in the curve, where the intensity for these days should appear. A great disturbance spoils somewhat the form of the curve in March; the point belonging to the 29th of March would occur about 1½ inches below the margin.

20. The law of the variation of the earth's horizontal intensity with the moon's phase, is one productive of many speculations. There is an evident connexion of the great diurnal variations of the horizontal intensity, with reference to the sun's hour angle; there is also a strongly marked connexion between the diurnal range and the sun's altitude; and we have a certain connexion between the sun's declination and the annual period. Are these connected with the heating power of the sun, its light, or its magnetism? Sir John Herschel has stated, that, as the sun's rays shine with their whole force on the moon's surface for a fortnight, unstopped by an atmosphere, the heat of the surface must be much more intense than that of a tropical summer; while, after the next fortnight, the cold must be more severe than that of a polar winter. M. Cournot, the French translator of Sir John Herschel's Treatise on Astronomy, opposes this opinion, and argues that, as there is no atmosphere to prevent radiation, our knowledge of the laws of radiant heat would lead to the conclusion, that the temperature of the moon's surface would differ little at the times of new and full moon.* Supposing Sir John Herschel's opinion accurate, if we could conceive the moon as a magnetic body acting by induction on the earth, then, according to our knowledge of the effect of heat on magnetic bodies, its intensity would be greatest when it was coldest, and least when warmest: the period of greatest cold we should expect

* Quoted by M. Francoeur; Uranographie, p. 97.

to be a day or two after the new moon, and of the greatest heat a day or two after full moon, in the same way as our periods of greatest cold and heat are after the winter and summer solstices. This seems to agree with the periods of maximum and minimum horizontal intensity. If M. COURNOT be right, or if Sir JOHN HERSCHEL'S supposition be insufficient, then we must look to the solar emanations reflected or radiated from the moon for the causes of the variations of the earth's magnetism, and to our atmosphere for a cause of the supposed retardation of epoch.*

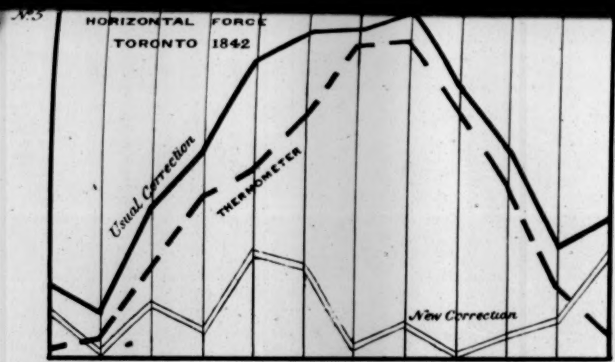
21. The connexion of lunar phase and horizontal intensity was first noticed by me in July 1845. I am not aware of any investigations on the relation of the horizontal intensity to the lunar month, excepting a paper by M. HANSTEEN, of which I have lately merely seen the title, which refers to the connexion of the horizontal intensity with the moon's ascending node.

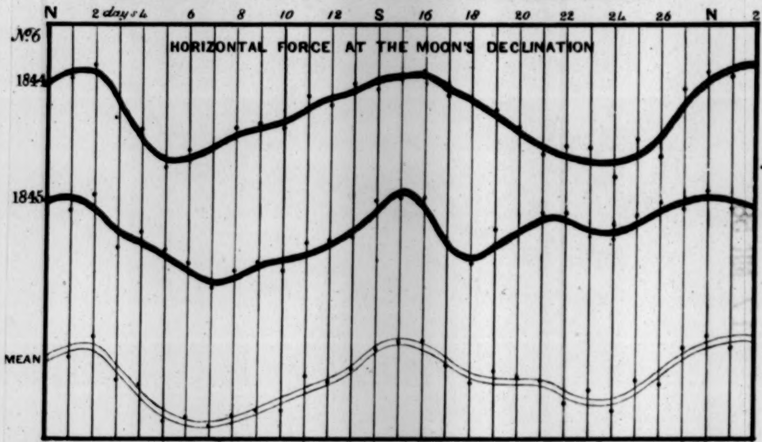
22. Having mentioned some time ago to Professor FORBES, that I was engaged in examining the relation of the lunar periods to the variations of the earth's magnetism, I learned from him that M. KREILL of Prague had stated, in his volume of observations for 1842, that the horizontal intensity was greater at the moon's passage of the inferior meridian, than at its passage of the superior meridian. I know not whether M. KREILL has verified his statement, or to what extent his observations prove it.† I have now discussed the observations for 1844, with reference to this period, and have verified my results by a similar discussion of the observations for 1845. I shall, at present, merely state the leading facts, and leave the details to another communication.

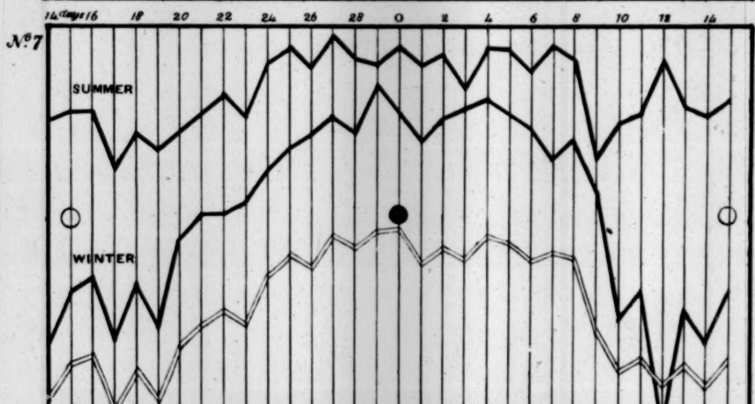
23. The observation at the hours on which the moon was on the meridian were termed 0 hours, the observation the hour after one hour, and so up to 24; as the moon takes about 25 hours to return to the meridian again. On some occasions there were only 24 observations between the two passages; in these cases (few in number) the hour of passage was reckoned as 24 hours, and also as 0 hours of the next day. The summations for the hours were made for each month; I shall only speak of the means for the whole year in this communication; these means have been projected. The large disturbances have been eliminated from the summations for 1844 and 1845. (See Curves, No. 9, Plate IV.) Any observation in 1844 which shewed a difference from the monthly mean, for the hour at which

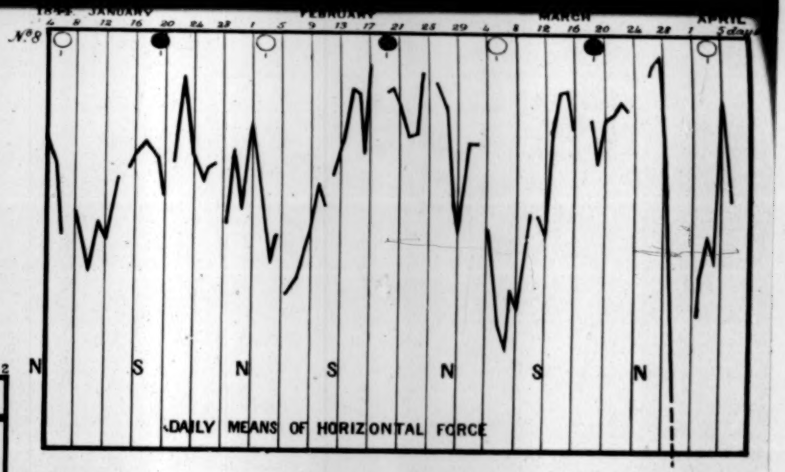
* It is evident that the variations of horizontal intensity may be due either to changes of the total intensity, or of its direction; any reasoning, therefore, on these facts must be necessarily incomplete, until we are certain of the actual effect.

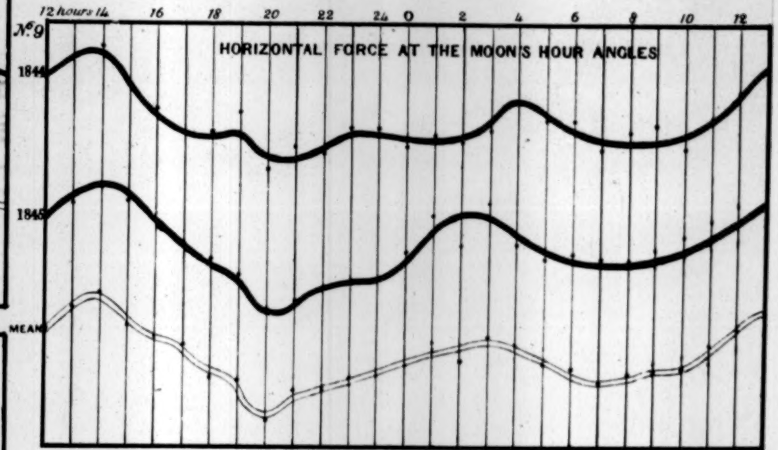
† I have, since this was written, been favoured by Professor FORBES with a copy of M. KREILL'S table for the horizontal force during the moon's hour angle. It indicates a minimum of intensity about two hours before the meridian passage, and maximum peaks at 12^h and 15^h, giving the interpolated period of maximum about $1\frac{1}{2}$ hours after the inferior meridian passage; the latter period agrees completely with my own conclusion, the former differs about three hours from my result.











CURVES

to illustrate changes of

HORIZONTAL MAGNETIC FORCE

at

MAKERSTOUN.

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it was made, of above twenty scale division (twelve times the resulting range of horizontal force in the lunar hour angle curve), was rejected, and an interpolated quantity substituted; this elimination, however, was not found to affect the periods of maxima and minima; it reduced the range, and rendered the curve somewhat more regular. In the observations for 1845, as a farther check on these eliminations, a different test number was employed, namely, forty scale divisions, or nearly twenty-five times the resulting range of the lunar hour angle curve.

24. In both years, *the* minimum occurs at 20^h, or 5^h before the meridian passage; *the* maximum at 14^h, or about $1\frac{1}{2}^h$ after the inferior passage: in both years, *a* minimum occurs at 8^h; in 1844, *a* maximum occurs before 4^h; and in 1845, before 3^h. The maximum at 3^h, for 1845, differs little from the maximum at 14^h; but the maximum at 4^h, in 1844, is considerably less than the maximum at 14^h. The coincidences of these results may be considered extraordinary, when it is known that the range in 1844 is only 0·000211, and in 1845 only 0·000213, or less than the effect of one degree of Fahrenheit on the magnetism of the bifilar bar.

25. Several questions spring from this result of the connexion of the intensity with the moon's hour angle. Does the range of the lunar hour angle curve vary with the moon's declination? If so, then we do not eliminate the lunar effect from the solar day curve by a monthly or any other summation. Do the periods of maxima and minima vary throughout the lunar month in the lunar hour angle curve, as they do through the year in the solar day curve? These questions I shall endeavour to examine at another opportunity.

MAKERSTOUN, *December* 26. 1845.

XIII.—*On the Decomposition and Dispersion of Light within Solid and Fluid Bodies.* With a Plate. By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S., and V.P.R.S. Edin.

(Read 2d February 1846.)

HAUY*, and other mineralogists, observed the two colours which are visible in several varieties of fluor-spar. He regarded the two tints as complementary, and explained them, as he did every other analogous phenomenon, by a reference to the colours of thin plates. In describing a species of dichroism noticed by Dr PROUT† in the purpurates of ammonia and potash, Sir JOHN HERSCHEL ascribes the green reflected light‡ "to some peculiar conformation of the green surfaces producing what may be best termed a *superficial colour*, or one analogous to the colour of thin plates, and striated or dotted surfaces." And he adds—"A remarkable example of such superficial colour, differing from the transmitted tints, is met with in the green fluor of Alston Moor, which, on its surfaces, whether natural or artificial, exhibits, in certain lights, a *deep blue* tint, not to be removed by any polishing."

Having, many years ago, found the same property in the Derbyshire fluor-spars, I was led to study it with particular attention; and, in 1838, I communicated the results of my observations to the British Association at Newcastle.§ In every specimen in which the colour in question exists, I found it to arise from *internal*, and not from *superficial* reflexion. In an extensive series of experiments on the absorption of light by the aqueous and alcoholic solutions of the colouring matter of plants, I found this property of internal dispersion in thirty or forty of these solutions. The most remarkable of these was the alcoholic solution of the colouring matter of the leaves of the common laurel. At first its colour is a bright green, afterwards changing into a fine olive colour; but in all its stages it disperses light of a *brilliant blood red colour*, which forms a striking contrast with the transmitted tint. After a long exposure to light, the transmitted tint almost wholly disappears, while the dispersed light retains its red colour.|| Another

* Traité de Mineralogie, tom. i., p. 512, 521.

† Philosophical Transactions, 1818, p. 424.

‡ Treatise on Light, art. 1076.

§ See Report of the Eighth Meeting, and Trans. of Sections, p. 10-12.

|| I shewed this experiment in 1836, at Lacock Abbey, to Mr FOX TALBOT, and several members of the British Association. At the meeting of the British Association at Manchester, in 1842, a friend handed to me, in the sectional meeting, a "solution of stramonium in ether," which

very remarkable example of internal dispersion, pointed out to me by Mr SCHUNCK, is exhibited in an alkaline, or in an alcoholic solution of a resinous powder produced from *orcine* by contact with the oxygen of the air. Its colour by transmitted light is reddish brown, and the light which it disperses is of an exceedingly rich *green* colour.

Since these experiments were made, my attention has been called to two interesting papers by Sir JOHN HERSCHEL, in the last part of the Philosophical Transactions; the one *on a case of superficial colour presented by a homogeneous liquid internally colourless*, and the other *on the epipolic* (or *superficial*) *dispersion of light;* and as these papers contain results incompatible with those which I had previously published, I found it necessary to resume the investigation of the subject.

The two papers now referred to are chiefly occupied with a description of the phenomena of coloured dispersion, as exhibited in a diluted solution of *sulphate of quinine* in weak sulphuric acid. Owing to the solution being nearly colourless by transmitted light, the general phenomenon is very beautiful. The line of bright blue light dispersed by the stratum of fluid immediately beneath the surface of incidence, and about the 50th of an inch thick, *appears* to be confined to that stratum, and it is in this respect only that the phenomenon differs from that which is exhibited by fluor-spar and the vegetable solutions which I have mentioned.

1. *On the Internal Dispersion of Fluor-Spar.*

There are many varieties of fluor-spar in which no dispersion of the intromitted light takes place. It does not exist in the *yellow*, *red*, and bright *blue* varieties which I have examined. It occurs chiefly in the *green* fluor from Alston Moor, and in several *pink*, and *bluish-yellow* varieties from Derbyshire. In order to observe the phenomena of dispersion most distinctly, I transmit a condensed beam of the sun's light through the specimen, when partially covered with black wax or black velvet. In some specimens, the intromitted beam is partially dispersed in a fine blue tint from every part of the solid which it traverses; but in other specimens, which are composed of strata of different colours, parallel to the faces of the cube, a very different and a very instructive phenomenon is displayed. The intromitted beam A B C, Fig. 1, Plate V., is crossed with bands of dispersed light of different colours, and of different intensities. In one case, a *pink* light was dispersed from the stratum close to the surface of incidence; from the next stra-

dispersed a *bright green light*. I described the phenomenon to the meeting, and it is noticed in the Transactions of the Sections, p. 14. Upon making the solution myself, I cannot obtain the same tints, either from the stalk or the dried leaves of the plant. The solution of the leaves disperses a brilliant red tint, like that mentioned in the text. The solution put into my hands must, therefore, have been one of the seeds of stramonium, or of some other substance possessing internal dispersion in a high degree.

tum there was *no dispersion* at all; this was followed by a narrow stratum, which dispersed a *bright whitish light;* then succeeded a stratum of non-dispersing fluor, and alternately dispersing and non-dispersing strata, scattering the fine blue light which has already been mentioned.

These results, which I have shewn to different persons, are incompatible with those obtained by Sir John Herschel with the very same variety of *fluor-spar*. He regards the blue dispersed light as *strictly* an *epipolic* or *superficial* tint,—so superficial, indeed, "that it might be referred to a peculiar texture of the surface, the result of crystallization, were it not that it appears equally on a surface artificially cut and polished."* Were I to hazard a conjecture respecting the cause of this difference in our results, I would ascribe it to the different degrees of light in which the observations were made. While I used a condensed beam of the sun's light, Sir John Herschel seems to have employed chiefly the ordinary light of day. In studying the phenomena in the solution of quinine, he "exposed it to strong day-light or sunshine;" and in another experiment, which pre-eminently required a powerful illumination, he "directed a sunbeam downwards on the surface, by total reflection from the base of a prism," which was in reality inferior to the ordinary sun's light. In the case of fluor-spar, however, he states that the epipolic colour is seen in perfection when "exposed to daylight at a window." In such a feeble light I could not have seen the phenomena I have described, and it is owing chiefly to the intensity of the light which I employed, that I have been enabled to place it beyond a doubt that the blue light dispersed by fluor-spar is reflected from every part of the interior of the crystal, and is not produced by any action either strictly or partially superficial, or solely by any stratum near the surface.

Sir John Herschel mentions, that the green fluor-spar of Alston Moor is the only solid in which he has observed an epipolic tint. It is the only mineral in which I have found an internal dispersion, excepting, of course, the minerals which exhibit the analogous phenomena of opalescence and chatoyance; but I have found several glasses which possess it, one in particular of a *yellow* colour, which disperses a *brilliant green* light, and another of a *bright pink* colour, which also disperses a *green* light, and a third of an *orange* colour, which disperses rays of a *whitish green* colour. In these cases, the glass has a decided colour of its own; but I have found many specimens, both of colourless plate and colourless flint glass, which disperse a beautiful green light.

2. *On the Internal Dispersion of the Solution of Sulphate of Quinine.*

Sir John Herschel describes the epipolic dispersion of this solution as "occupying a very narrow parallelogram, having a breadth of about a 50th of

* Philosophical Transactions, 1845, p. 143.

an inch, of a vivid and nearly uniform blue colour over its whole breadth;"* but upon "directing a sunbeam downwards on the surface, by total reflection from the base of a prism, a feeble blue gleam was observed to extend downwards below this vivid line to nearly half an inch from the surface, thus leaving it doubtful whether some small amount of dispersion may not be effected in the interior of the medium at appreciable depths." By using condensed solar light, this doubt is immediately removed, and the phenomenon ranks itself as one of internal dispersion, differing only in the law of its intensity from those which I have already described. In the one the dispersible rays are thrown *gradually*, in the other *quickly*, from the intromitted beam,—a phenomenon to a great extent identical with what takes place in the analogous phenomena of absorption.

If the dispersing action of the solution were rigorously confined to a stratum the fiftieth of an inch thick, it would have followed, of necessity, that "*an epipolized beam of light* (meaning thereby, a beam which has been once transmitted through a quiniferous solution, and undergone its dispersing action) *is incapable of further undergoing epipolic dispersion*;" but as the dispersing action is not thus limited, that conclusion must be incorrect. Sir JOHN HERSCHEL, indeed, has deduced this result from direct experiment with a plate of glass immersed vertically in a quiniferous solution. In this case he could perceive no trace of colour either at the ingress or egress of the epipolized beam which was incident upon the plate. Sir JOHN does not mention the distance of the plate from the epipolising stratum. If the distance was small, we are confident, from direct experiment, that the blue tint would have been seen; but if the distance was considerable, then the beam, incident upon the glass, must have been previously shorn of all its dispersible rays.

In examining the blue rays themselves, Sir JOHN found that they consisted of a "small per-centage of rays, extending over a great range of refrangibility." They formed, however, a continuous spectrum deprived of the less refrangible red, nearly of the whole orange, and all the yellow; a rich and broad band of fine green light, slightly fringed with red, passed into a copious indigo and violet without the intermediate blue.

The comparatively feeble light of the dispersed blue rays renders it difficult to ascertain their susceptibility of being a second time dispersed. Sir JOHN HERSCHEL could not obtain any indication of this susceptibility; but we have no doubt that with condensed light their second dispersion will be discovered: and we are led to this opinion by the fact, that Sir JOHN believed that the epipolic dispersion takes place in all directions, and therefore expected to discover a second dispersion under circumstances in which, according to my experiments, it could not be found.

* The best method of seeing this experiment, is to take the solution into the open air, where the whole light of a blue sky can fall upon its surface. I have in this way seen the blue line perfectly luminous at that stage of a December twilight when there was not light enough to read by. I consider, therefore, the light of the sky as peculiarly susceptible of this species of dispersion.

Fig. 1.

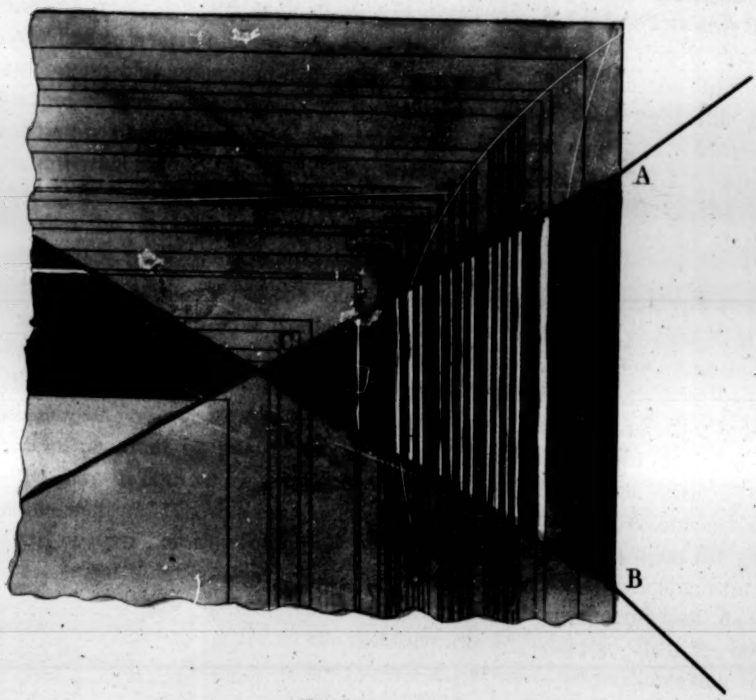
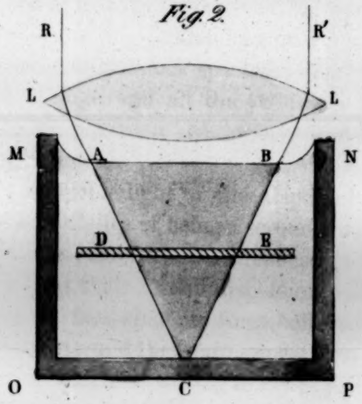


Fig. 2.



Fr. Schenck, Lith. Edin.r

Sir JOHN has clearly shewn, that the light is dispersed outwards as well as laterally; but as he was conversant only with the phenomena of a narrow blue line, and had not seen the blue cone of rays dispersed from the cone of condensed light, he could not be aware of the changes which take place in its colour while the eye passes from the azimuth of 90° to that of 100°.

These changes are very decided, and will be understood from fig. 2 (Plate V.), in which M N O P is a horizontal section of the vessel containing the solution; R R′ a beam of solar light, incident upon an achromatic lens L L, and condensed into the luminous cone A C B. Now, the blue colour produced by the first stratum, next to the side A B, is exceedingly strong, and that which occupies the rest of the cone A C B comparatively faint. When we view the bright blue stratum in the direction N M, or in the azimuth of 90°, the tint is very brilliant, because the eye receives all the blue rays dispersed by the whole length A B of the stratum; whereas, when we view it in the direction R′ C, in the azimuth of 0°, we only see the tint corresponding to the thickness of the stratum. The tint, however, is, in reality, a maximum in the azimuth of 0°, and gradually diminishes till it ceases in the azimuth of 180°, or in the direction C R′.

If we now immerse in the fluid a plate of colourless glass, whose section is D E, so as to receive the beam A B E D, we shall find that there is no peculiar dispersion, as Sir JOHN HERSCHEL observed, either at its surface of incidence or emergence. Hence he concluded that the epipolised beam A B E D "is incapable of undergoing farther epipolic dispersion;" and that having thus "lost a property which it originally possessed, it could not, therefore, be considered *qualitatively* as the same light."

Now, in using a condensed beam of light, as we have done, we find that the whole cone A B C, even when *two* inches long, and with a December sun, disperses the blue light, and the stratum behind the glass plate D E nearly as much as the stratum before it. In fluor-spar, and in the other fluids I have mentioned, this is still more strikingly the case,* and hence neither of the conclusions drawn by Sir JOHN HERSCHEL are admissible.

The following appear to me to be the deductions which the experiments actually authorize :—

1. A beam of light which has suffered dispersion by the action of a solid or fluid body, (that is an *epipolised* beam) is capable of further undergoing epipolic

* In one of these experiments a piece of green fluor, from Alston Moor, when immersed in the quiniferous solution, dispersed a fine *violet blue* light, at the distance of *three-fourths* of an inch from its surface. In another experiment, a beam of light that had been dispersed in the solution of quinine, again suffered dispersion at *two inches* distance from the surface of a piece of Derbyshire fluor.

A beam of light that has passed through the Esculine solution disperses blue light, but not copiously, when transmitted through the quinine solution; but the beam that has passed through quinine is copiously dispersed when transmitted through Esculine.

dispersion, provided the thickness of the medium is not so great as to have dispersed all the dispersible rays.

2. When such a medium is thus rendered incapable of dispersing more light, it is not because it has lost a property which it originally possessed, but because it is deprived of all the dispersible rays which it contained.

It is no doubt an interesting fact, that a small number of differently coloured rays, constituting blue light by their mixture, should possess this property of being dispersed, while other rays of the same refrangibility are either less dispersible, or apparently indispersible, by the same medium; but the fact will appear less surprising and anomalous when we advert to certain phenomena of absorption in which the same property is displayed.

The difference between the *absorption* and the *internal dispersion* of light is simply this. In the one case the portion of light withdrawn from the intromitted beam is *extinguished* and *invisible*, and in the other *dispersed* and *visible*; and we may compare the two classes of phenomena by *supposing* that the light extinguished by absorption is rendered visible as if by dispersion. Now it is a remarkable fact, that almost the whole of the blue light absorbed by the mineral called *native orpiment* is extinguished during the passage of the light through the first stratum, whose thickness is less than the fiftieth of an inch; and hence it is that the thinnest slice of this substance has nearly as deep a yellow colour as the thickest. Were the absorbed blue rays to become visible by dispersion, we should actually see a more striking example of epipolism, or dispersion confined to the first stratum, than in the quiniferous solution. Even the condensation of the beam would not in this case give us a blue cone of light.

The analysis of the blue line indeed would indicate a difference between the two phenomena. It would shew that the blue light was derived chiefly from the *violet*, *indigo*, and *blue* spaces, and but partially from the *green*, *yellow*, *orange*, and *red*, having appropriated the whole of the more refrangible rays, and but a very small portion of the less refrangible ones; whereas the blue light from the quiniferous solution is derived almost in equal proportions from all the coloured spaces excepting the least refrangible, red. The limitation of the rays capable of absorption, like the limitation of the dispersible rays in the quiniferous solution, is shewn in the action of various bodies on the spectrum. Such bodies change the colour of certain spaces in the spectrum, without continuing to absorb the residual rays; that is, when the absorbable rays are removed by a certain thickness of the body, an additional thickness operates very feebly, as in the quiniferous solution, in altering the colour of the residual beam.

I have pointed out these analogies between the phenomena of absorption and dispersion to meet the case of the bright blue line in the quiniferous solutions. The dispersion of fluor-spar, and of the glasses and vegetable solutions already described, is of a different character. In fluor-spar the dispersion effected

by the first stratum is by no means very abundant, and the intromitted beam, even after passing through one or more undispersing strata, is dispersed nearly as copiously as before. In the glasses and in the vegetable solutions there are no peculiarities which require explanation, excepting those which arise from the absorption of the dispersed beam in passing through the coloured medium.

When the phenomena of internal dispersion are exhibited in coloured fluids and solids, the influence of absorption upon the dispersed light is very interesting. Previous to its dispersion the light has the same colour as the transmitted light, were it to emerge at that point of its path, and when viewed at an azimuth above 90°, a portion of the dispersed light has that colour. The quantity of light possessing this colour increases between the azimuth of 90° and 180°. In order to see this effect disembarrassed from another influence, we must make the intromitted beam parallel to the surface of the fluid or solid, so as just to graze it. In this way the dispersed light is not changed in its passage to the eye after dispersion. When the beam passes through the coloured medium without this precaution, it again suffers absorption proportional to the thickness of the coloured substance through which it has passed, and sometimes disappears altogether. This effect is finely seen in the darker solutions, which disperse a brilliant *red*, or a brilliant *green* light; the colour of the former becoming *yellowish green* and *whitish*, while that of the latter becomes *whitish yellow*.

3. *On the Polarisation of Dispersed Light.*

As the dispersed light is turned from its path by reflection, and is reflected at angles proper for polarising it, its partial polarisation at least might have been anticipated. Sir John Herschel viewed it through a tourmaline, and states that no signs of polarisation were perceived in it; but his method of obtaining the blue line from light diverging from a large area of the sky, and therefore reflected at various angles far above and far below the polarising angle, rendered it impracticable to detect its state of polarisation. The method which I adopted, of using a narrow cylindrical beam of strong light, affording a bright dispersed beam more than an inch in length, enabled me to discover its polarisation, and to investigate its peculiarities.

Upon examining the blue beam in the quiniferous solution with an analysing rhomb of calcareous spar, I found that a considerable part of it, consisting chiefly of the less refrangible portion of its rays, was polarised in the plane of reflection, while the more refrangible of its rays, constituting an intensely blue beam, had a different state of polarisation.

This insulation of the bluer rays greatly increased the beauty of the phenomenon, and promised to throw some light upon its cause. I was therefore anxious to ascertain their state of polarisation, which was not indicated by the analysing rhomb. With this view I transmitted through the solution a strong beam of polarised

light, and was surprised to find that the blue beam which it yielded by dispersion, retained the same intensity in every position of the analysing prism, and therefore possesses a *quaquaversus* polarisation, such as that which light receives when transmitted through a congeries of minute doubly refracting crystals having their axes in all possible directions.

In making the same experiment with other dispersing fluids and solids, I found some in which the whole beam was completely polarised in the plane of reflection, and others in which it exhibited solely a *quaquaversus* polarisation; but as these experiments indicate new processes in the decomposition and polarisation of light, which require a more extended analysis, I shall resume the subject in a separate communication, contenting myself at present with a general account of the more important facts, and the results to which they lead.

Having transmitted a condensed beam of light through an alcoholic solution of the leaves of the Common Laurel, or of Tea, either green or black, I found that the *bright red beam* which it dispersed, possessed, like the *blue* one in the quiniferous solution, a *quaquaversus* polarisation, a small portion of the light being polarised in the plane of reflection. The *green* beam dispersed by the preparation of *orcine*, has the same properties, the white portion of it disappearing and reappearing during the revolution of the analysing rhomb. In the aqueous solution of *esculine*,* the dispersed pencil consists of two finely-contrasted pencils, the one *whitish* and polarised in the plane of reflection, and the other a *very deep blue*, having *quaquaversus* polarisation. The *white* pencil is more intense than the *blue* one, which is the very reverse of what takes place in the solution of quinine. The alcoholic solution of the seeds of the *Colchicum autumnale* gives a bright and copious *green* beam of dispersed light, which consists of two pencils, one whitish and polarised in the plane of reflection, and the other bright green, with a quaquaversus polarisation. The same property is possessed by a solution of *guiacum* in alcohol, which disperses, by the stratum chiefly near its surface, a beautiful *violet* light; and also by an alcoholic solution of *sulphate* of *strychnine*, which disperses a green light, *after it has stood for some days.* The same property is possessed by almost all the oils, in some of which the dispersed light is exceedingly beautiful, varying from a pale green to a blue tint.

The polarisation of the dispersed beam in one plane, namely, in the plane of reflection, is exhibited in several fluids and solids. It is very marked in the bile of the ox, which disperses an olive-green light; in a solution of gum-myrrh in alcohol, diluted with water, which disperses a bright white beam; and in an orange-coloured glass, which disperses a pale greenish beam.

In many fluid solutions, the beam with a *quaquaversus* polarisation is very intense, when compared with the faint pencil which is polarised in the plane of

* In the alcoholic solution of Esculine, the *faint-blue* approaches to *violet.* The polarisation is like that in quinine.

reflection; but in a specimen of *yellow Bohemian glass*, which gives a copious and brilliant *green* beam by dispersion, the whole of the beam possesses a *quaquaversus* polarisation.

When we view the dispersed beam in different azimuths, some very interesting phenomena present themselves to our notice. In general, the colour of the dispersed light suffers a considerable change, passing, between the azimuths of 90° and 180°, from the colour of the dispersed beam to the colour of the transmitted beam. This effect is finely seen in the alcoholic solution of tea, where the brilliant *red* light passes into an *olive* tint; but it is still more remarkable in a mixture of *Prussian blue* and water. The dispersed beam is polarised in the plane of reflection. It is *bluish* in the azimuth of 90°: *pinkish* about the azimuth of 100°; *greenish* in that of 120°; *bluish* in azimuth 150°; and again *pinkish* in azimuth 170°. These three last tints may be all seen at the same time.

Such are the general phenomena of internal dispersion, a subject which promises to throw some light on the constitution of those solid and fluid bodies by which it is produced. The *apparently superficial dispersion* in the quinine solution to which Sir JOHN HERSCHEL has given the name of *epipolism*, is obviously a single case of the general phenomenon in which the ordinates of the curve of dispersion diminish rapidly after the light has entered the stratum nearest the surface; while the *real epipolism* which he ascribes to fluor-spar, so far from being an action of the surface, is much less so than that of the quiniferous solution, and entirely similar in its character to that which is produced by the fluids and solids which I have examined.

The phenomenon of internal dispersion, when considered merely as a case of reflection and polarisation, possesses much novelty and interest. If the exciting beam, as we may call it, is cylindrical, we have before us an experiment, in which the phenomena of *cylindrical reflection*, and *cylindrical polarisation*, are at once exhibited to us. The innumerable reflecting surfaces, receiving the intromitted beam at all possible angles, reflect the incident light in all possible directions, so that the eye, wherever it is placed, sees the beam as if it were self-luminous; and while the eye is made to revolve in a circle round the cylindrical beam, it receives a pencil of polarised light—polarised in a plane passing through the eye and the axis of the cylinder; or, what is the same thing, a thousand spectators viewing this beam in the same azimuth, but in directions differently inclined to the horizon, would all see exactly the same phenomena of reflection and polarisation!

4. *On the Causes of the Internal Decomposition and Dispersion of Light.*

In imperfectly crystallized minerals, such as particular specimens of *adularia, chrysoberyl, opal,* and *sapphire,* the white and coloured opalescence, and the asterial radiations, have been shewn to arise from minute vacuities, or from open spaces with crystallized sides, or from narrow pipes, or linear spaces parallel to

the edges of the primitive or secondary forms of the mineral. In tabasheer, where the vacuities contain air, which we can expel and send back at pleasure, a fine blue light is dispersed, depending, no doubt, on the size of the vacuities. In a very remarkable specimen of calcareous spar, crowded with hemitrope veins, I have observed a copious internal dispersion produced by the reflection of light at the different surfaces, which, though in optical contact, have different degrees of extraordinary refraction.

All these phenomena, however, are essentially different from those which form the subject of this paper, with the exception of the phenomena of fluor-spar, in so far, at least, as they are the result of imperfect crystallization. The *epipolism* which Sir JOHN HERSCHEL ascribes to this mineral, or its *internal dispersion*, according to my experiments, does not belong to the species, but only to particular varieties, and not even to the variety, but merely to particular parts of it. It is therefore the result of inequal or imperfect crystallization. The nucleus is perfect, a coating supervenes, having a different tint by transmitted light, and dispersing a fine blue light, and so on through a succession of strata, dispersing differently coloured lights, and separated by non-dispersing spaces. An extraneous element, therefore, depending on the state of the solution, has been successively introduced into the crystal, and if it had the same refractive and dispersive power as the fluor-spar, it could not reflect any portion of the intromitted beam: But if there is any difference in the mean refraction, or in the dispersive power, or if the difference consists merely in the unequal length of certain portions of the two spectra, then, in all these cases, light will be dispersed by the extraneous element. If, for example, we place a film of oil of cassia between two prisms of flint glass, the light reflected from the film will be *blue*. The index of refraction for certain of the *red* rays is the same in the glass and in the oil, and consequently none of these rays enter into the reflected pencil, which must therefore be *blue*, whatever be the inclination of the incident rays. If we now suppose this film of oil to be solidified, and disseminated in infinitely smali atoms throught flint glass, or a fluid that has the same action as the glass upon light, we should have the phenomenon of a blue dispersion.*

A beam of blue light thus produced should be polarised at the polarising angle, and partially polarised at other angles; and if this is not its character, we must look for some cause by which it has been counteracted. We have already seen that, in the Bohemian yellow glass, none of the light is polarised by reflection, and that in the quiniferous solution only a part of it is so polarised, the whole pencil in the one case, and the residual pencil in the other, having a *quaquaversus* polarisation. This effect cannot be the result of an opposite polarisation

* In the experiment with *Prussian blue*, which is a very splendid one, the particles are mechanically suspended in the water; so that we have here an ocular demonstration that the particles are the cause of the dispersion and the *quaquaversus* polarisation.

by the refraction of the dispersed light at the surfaces of the reflecting particles, because such an action would only reduce the amount of polarisation by reflection; and I have found by direct experiment, namely, by making the blue light pass through different thicknesses of the fluid, that such an effect is not produced. Unless, therefore, we suppose that this *quaquaversus* polarisation is a new property of light, produced by a peculiar action of certain solid and fluid bodies, we are driven to the conclusion, no less remarkable, that it is produced by an infinite number of doubly refracting crystals, having their axes of double refraction lying in every possible direction, and therefore reflecting from their posterior surfaces a pencil of light with quaquaversus polarisation.

ST LEONARD'S COLLEGE, ST ANDREWS,
January 30. 1846.

XIV.—*On the Constitution and Properties of Picoline, a new Organic Base from Coal-Tar.* By Thomas Anderson, M.D.

(Read, 20th April 1846.)

The careful study of the products of destructive distillation has enriched organic chemistry with an extensive series of results of unexpected interest and importance. These results have affected, in no inconsiderable degree, the recent progress of the science; and their influence has been of a twofold character, both general and particular, exerted in the former case in the development of some of the more remarkable general doctrines of organic chemistry; in the latter, in the important light thrown by their investigation on the constitution of the substances from which they are derived, and the facilities they have afforded of following out connections, which the examination of the original substance either does not at all present to our view, or, at least, indicates only in an imperfect or dubious manner. Added to this, we have the remarkable fact of the appearance among these products of substances in some cases identical with those occurring in organised beings; and in others, presenting analogies of the very closest character with the actual products of vital affinity, which, taken together, afford abundant reason for pursuing the investigation of substances which have already afforded results of so remarkable a character.

Setting aside altogether those substances, the occurrence of which is so frequent, that they may be called the general products of destructive distillation, such as carbonic acid, light carburetted hydrogen, olefiant gas, acetic acid, &c., it may be laid down as a general rule, that each individual compound produced during such a process, is formed by the destruction of a limited number of substances only, which bear to each other, and to the product, a more or less intimate connection in constitution or chemical relations. In those instances in which we have been enabled to submit to destructive distillation substances of a definite and simple constitution, in a state of chemical purity, and where an uniform temperature has been preserved, the results have been, for the most part, of an exceedingly simple and intelligible character; but in proportion as the atom becomes more complex, so also do the products of its decomposition, and the explanation of the results is found to be proportionately difficult and uncertain. These difficulties and uncertainties are increased in a still higher degree, in the case of a substance such as coal, where we have to deal not merely with one complex atom but with a congeries of several such, and where the process is performed on the large scale, and under a variety of perturbing influences. The distillation of coal is, in fact,

attended by the formation of about twenty different substances, the constitution and properties of which have been examined with different degrees of accuracy, and which present among them instances of almost every species of chemical compound. The discovery of six of these substances is due to RUNGE,* who published, about fourteen years ago, a very interesting memoir, containing an account of their general properties. Of these substances, three are possessed of acid properties, and three are bases, to the latter of which he gave the names of Kyanol, Leukol, and Pyrrol, from the peculiar colours developed by the action of certain reagents on their salts. The two former of these substances were afterwards submitted to a detailed examination and analysis by HOFFMAN,† who arrived at the interesting result, that both are identical with substances which had been independently obtained by the decomposition of certain well known bodies; Kyanol possessing the constitution and properties of the Aniline of FRITSCHE, and the Benzidam of ZININ; while Leukol is identical with the substance described by GERHARDT under the name of Chinoline, and which was obtained by him as a product of the distillation of quinine, cinchonine, and strychnia, with caustic potass. HOFFMAN failed, however, entirely in obtaining any evidence of the presence of pyrrol in the substance which he examined, and leaves in doubt the existence of such a compound.

Having lately had occasion to examine a quantity of the mixed bases contained in coal-tar, obtained by a method similar to that of RUNGE, but which, owing to a modification of the process, contained all the more volatile bases formed during the distillation of coal, I was led to try whether or not pyrrol was to be found in it, and I obtained immediate evidence of its existence, by the characteristic red colour which it gives to fir-wood moistened with hydrochloric acid. The attempt to separate this pyrrol proved that it was present in extremely minute quantity only, but led to the discovery of a new base different from those of RUNGE, for which I propose the name of Picoline, and the examination of whose properties forms the subject of the present paper.

Preparation of Picoline.

For the crude substance employed in the preparation of picoline, I am indebted to the kindness of Mr ASTLEY, of the Bonnington Chemical Works, and it was obtained by the following modification of RUNGE's process. In the preparation of naphtha from coal-tar, the first product of distillation is agitated with sulphuric acid for the purpose of separating any naphthaline which may be present, as well as a variety of substances in extremely minute quantity, which communicate to the crude naphtha the property of becoming dark-coloured

* Poggendorf's Annalen, Band 31, u. 32.

† Annalen der Chemie und Pharmacie, vol. xlvii..

by exposure to the air; among these substances, of course, are all the basic compounds contained in the oil. The sulphuric acid which had been used for this purpose was neutralised by impure ammonia obtained by a single distillation of the watery fluid of the gas-works. On the addition of the ammonia there was no separation of any oil in quantity appreciable to the eye; but upon distillation, the bases, which had been dissolved in the fluid, passed over with the first portions of water, and collected in a separate layer in the receiver. This oil, when it came into my hands, possessed a very dark brown colour, a somewhat viscid consistence, and a peculiar pungent and disagreeable odour. It was heavier than water, a layer of which, containing a small proportion of oil in solution, floated on the surface. The examination of this oil proved it to consist, in addition to picoline, of a mixture of pyrrol, aniline, an oily base possessing the general properties of leukol, and a thick heavy oil destitute of basic properties.

In order to separate picoline, the oil, along with the water which floated on its surface, was introduced into a retort and carefully distilled. At first, water, accompanied by a little oil, passed over, and then an oil by itself, which dissolved completely in the watery fluid contained in the receiver. As the distillation proceeded, another oil made its appearance, which collected in a layer on the surface of the fluid which had previously distilled. When about three-fourths of the oil had passed over, the process was stopped, by which means the oil, destitute of basic properties, which requires a very high temperature for its distillation, was left behind in the retort. The fluid in the receiver was now supersaturated with sulphuric acid diluted with water, care being taken to obtain a powerfully acid reaction. The peculiar odour which the fluid possessed, was by this process entirely changed, but not destroyed; and, on distillation, the water which passed over, carried with it all the pyrrol contained in the solution, while the other bases were retained by the sulphuric acid. Caustic potass was then added to the residue in the retort until an alkaline reaction was manifest, and it was again distilled; the water which passed over carried with it the oily bases, partly dissolved, partly floating on the surface of the solution, exactly as in the first distillation. A few sticks of fused potass were introduced into the product, and the whole was left in repose; as the potass dissolved, the oil, which is entirely insoluble in solutions of the fixed alkalis, rose to the surface and there collected in the form of a pale yellow layer, still containing a considerable quantity of water, which may amount to 30 or 40 per cent. of the bulk of the oil. The oil was separated from the watery fluid by means of a pipette and pieces of fused potass added so long as they continued to become moist. The dry oil was then introduced into a retort and distilled. A transparent and colourless oil passed over, which was tested at intervals by allowing a drop of it to fall into a solution of chloride of lime. So soon as the reaction of aniline made its appearance the receiver was changed. The first portion was now picoline in a state approaching to purity; that which

immediately followed, consisted of a mixture of picoline and aniline. The first portion was again digested with fused potass and rectified; that which distilled at 272° was collected apart, and constituted pure picoline.

Constitution of Picoline.

The general analogy in properties which picoline bears to aniline and the other oleaginous bases, permitted the assumption that it, like these substances, was free from oxygen; I proceeded, therefore, in its analysis, upon this hypothesis, and neglected the determination of the nitrogen. The following are the results of the analyses:—

Analysis I.	5·630	grains of	picoline gave
	15·954	...	carbonic acid,
	3·944	...	water.
... II.	5·347	grains of	picoline gave
	15·100	...	carbonic acid,
	3·670	...	water.

Which give the following results per cent.:—

	I.	II.
Carbon	77·16	77·18
Hydrogen	7·77	7·62
Nitrogen	15·07	15·20
	100·00	100·00

These results correspond closely with the formula $C_{12}H_7N$; the calculated result of which is—

		Theory.	Mean.
C_{12}	900·0	77·29	77·17
H_7	87·5	7·43	7·69
N	177·0	15·28	15·14
	1164·5	100·00	100·00

This formula is precisely the same as that of aniline, along with which picoline occurs in coal-tar. In order to ascertain whether the atomic weight of these substances were also identical, I prepared the platinum salt of picoline, and determined the amount of platinum contained in it. The salt was obtained by adding bichloride of platinum to a solution of picoline in excess of hydrochloric acid: no immediate precipitation took place unless the solutions were very concentrated, but in the course of twenty-four hours the salt was deposited in fine orange-yellow needles. When dried at 212°, it gave the following results:—

I. 9·670 grains of chloride of platinum and picoline gave
3·147 ... platinum = 32·544 per cent.

II. 10·814 grains of chloride of platinum and picoline gave
3·517 ... platinum = 32·522 per cent.

From these analyses are deduced the following atomic weights :—

I.	II.
1211·1	1213·7

These agree sufficiently well with the theoretical atomic weight, which is 1164·5. They correspond also precisely with the results of the analysis of the aniline salts. The identity of these results is shewn by the following table of the analyses by FRITSCHE, ZININ, and HOFFMAN, of aniline from its different sources, and of picoline, as well as of the plantinum salts of these substances :—

	Aniline.*	Benzidam.*	Cyanol.	Picoline.	Theory.
C =	77·73	77·32	76·67	77·17	77·29
H =	7·60	7·50	7·72	7·69	7·43
N =	14·98	14·84	15·62	15·14	15·28
	100·31	99·66	100·00	100·00	100·00

The following are the results for the platinum salts :—

	Benzidam.	Kyanol.	Picoline.	Theory.
Mean platinum, per cent.	32·501	32·886	32·533	32·94
Atomic weight .	1216·1	1170·5	1212·4	1164·5

The results of all these analyses agree perfectly with one another; but the properties possessed by picoline differ from those of aniline, which, whether obtained from coal-tar, indigo, or nitrobenzid, presents a perfect identity in its chemical characters.

Properties of Picoline.

Picoline is a perfectly colourless, transparent, limpid fluid, extremely mobile, and destitute of viscidity. It possesses a powerful, penetrating, and somewhat aromatic smell, which, when very dilute, is replaced by a peculiar rancid odour, adhering pertinaciously to the hands and clothes. Its taste is acrid and burning when concentrated; but when very dilute, as, for instance, when its vapour is sucked into the mouth, it is powerfully bitter, as are also the solutions of its salts. It is not changed by exposure to a cold of 0°. Picoline is extremely volatile, and evaporates rapidly in the air. It boils at the temperature of 272°, and the thermometer remains perfectly stationary during the whole period of the ebullition; it is, therefore, much more volatile than aniline, which, according to HOFFMAN, boils at 359°. It may be preserved for a long time in a bottle containing only a small quantity of it, and which is frequently opened, without becoming manifestly coloured; whereas aniline becomes rapidly brown, and, indeed, cannot easily be obtained colourless, except by distillation in a current of hydrogen. The specific

* Not having the original papers of FRITSCHE and ZININ at hand, I extract these two results from BERZELIUS' Arsberättelse, 1844, p. 454, where they are calculated according to C=75·12, the rest are with C=75, but the difference is so small as not to affect the comparison.

gravity of picoline is less than that of water. I found it to be 0·955 at 50°, while, according to HOFFMAN, that of aniline is 1·020 at 68°.

Picoline mixes with water in all proportions, and forms a transparent and colourless solution. It is insoluble, however, in solution of potass, as well as in most alkaline salts, the addition of which causes its immediate separation from the water. It dissolves also readily in alcohol, ether, pyroxylic spirit, and the fixed and volatile oils. It is a powerful alkaline base: a rod dipped in hydrochloric acid, and held over it, is immediately surrounded by a copious white cloud of hydrochlorate of picoline. It restores the blue colour of reddened litmus, but does not affect the colouring matter of red cabbage. It does not coagulate the white of eggs as aniline does.

The reactions which it produces with other substances are also quite distinct from those presented by aniline. When brought in contact with the solution of chloride of lime, it does not produce, in the least degree, the violet colour which is so characteristic of aniline; on the contrary, the solution remains perfectly colourless, unless, indeed, the picoline has not been well separated from pyrrol; in which case a slight brown makes its appearance, but no violet. Picoline is also incapable of producing the yellow colour in fir wood and the pith of the elder, which is so readily obtained with aniline. When treated with chromic acid, even when very concentrated, and after boiling, no change takes place in the colour of the solution, and only a small quantity of a yellow powder is deposited; while Aniline gives an abundant precipitate, which has, according to the degree of concentration of the fluid, a green, blue, or black colour.

Picoline precipitates from solutions of chloride of copper a portion of the oxide of copper, while the remainder forms a pale blue solution, which, when evaporated to a small bulk, deposits a congeries of prismatic crystals, which seem to be a double salt. No blackening of the solution takes place, as is the case with aniline. When an excess of hydrochloric acid is present there is obtained, on evaporation, another double salt in large crystals, apparently derived from the rhombohedral system. Picoline produces also double compounds with the chlorides of mercury, platinum, gold, tin, and antimony. With chloride of gold it gives an exceedingly characteristic compound, in the form of a fine lemon-yellow precipitate, which is soluble in a considerable quantity of boiling water, and is deposited, on cooling, in delicate yellow needles. Aniline, under similar circumstances, gives a reddish-brown precipitate resembling the ferrocyanide of copper. It gives, with infusion of nut-galls, a copious curdy precipitate of a pale yellow colour, which dissolves in hot water, and is deposited again on cooling. It does not precipitate the solutions of nitrate of silver, chlorides of barium and strontium, or sulphate of magnesia.

The properties of picoline, as now detailed, are obviously different from those of aniline. They recalled, however, strongly to my mind those of a base called

Odorin, obtained by UNVERDORBEN* from Dippel's animal oil. According to this chemist, Dippel's oil, which is obtained by several successive distillations of the oleum cornu cervi, is a mixture of four different bases, to which he gives the names of Odorin, Animin, Olanin, and Ammolin. Of these, the two first constitute nineteen-twentieths of the whole oil, and the odorin, which resembles picoline in its solubility in water, is obtained by simply distilling the oil and collecting the product as long as it dissolves. These results, however, have been called in question by subsequent observers; REICHENBACH, especially, asserts that he was unable to separate any basic compounds, and considers the substances obtained by UNVERDORBEN to be mixtures of empyreumatic oil with ammonia. As, however, the properties which UNVERDORBEN has attributed to odorin, approximate in some respects to those of picoline, I thought it desirable to ascertain the existence of this substance, and whether or not it is identical with picoline. In order to prepare odorin, I rectified the oleum cornu cervi, and then distilled the product; but on allowing the first drops of oil to fall into water, they were not dissolved as UNVERDORBEN has asserted, but floated unchanged upon the surface. Finding this process unsuccessful, I agitated the crude oil with dilute sulphuric acid; the acid fluid immediately acquired a very deep reddish-brown colour, and when separated from the oil, and supersaturated with potass, a semisolid viscid mass separated from the fluid. This, when distilled with water, yielded a mixture of several oily bases, while a dark coloured resinous substance, probably UNVERDORBEN'S Fuscin, was left in the retort. The mixed bases which I thus obtained, formed an exceedingly small fraction of the oil employed. They were purified by several successive rectifications, and generally in a method similar to that employed for picoline, and the first portions of the product collected apart. It then constituted a colourless oil which became brown in the air, dissolved readily in water, and presented an odour similar to, though not quite the same as, that of picoline. It gave with chloride of gold a dirty yellow precipitate, which dissolved in hot water, and deposited, on cooling, in the pulverulent form, and with bichloride of platinum, a compound in red wart-like crystals. By an accident in the laboratory, the small quantity of this substance which I had prepared for analysis was destroyed, so that the evidence of their identity cannot be considered as sufficient. The characters of odorin, as given by UNVERDORBEN, are not perfectly identical, either with those of picoline, or the base which I obtained. Odorin, according to UNVERDORBEN, boils at about 212°, and its salts are oleaginous compounds which distil in the form of an oily fluid, whereas those of picoline are mostly crystallizable. I am at present engaged with the examination of these substances.

It is obvious, from the observations contained in HOFFMAN'S† paper, that

* Poggendorf's Annalen, vol. xi.

† Liebig's Annalen, vol. xlvii.

picoline must have been present along with aniline and chinoline in the substance which he examined. He mentions, especially, that his aniline, as obtained by distillation only, possessed a peculiar pungent and disagreeable odour, which was got rid of only by several successive crystallizations of its oxalate from alcohol, and that the impure aniline has a specific gravity less than that of water. He observes, also, that the quantity of the substance present must have been excessively minute, as it did not affect the results of the analysis, a phenomenon, the cause of which is sufficiently explained by the identity in constitution of the two substances. HOFFMAN did not obtain picoline in the separate state, simply because the bases employed by him were obtained from the less volatile portions of coal-tar, which necessarily contain it only in minute proportion.

Combinations of Picoline.

Picoline forms a series of compounds which are generally closely analogous to those of aniline, but present in a less marked degree the regularity and facility of crystallization which are so characteristic of the salts of the latter base. It forms, however, with the greater number of acids, salts which can be obtained in a crystalline form. These are all highly soluble in water, and some of them are even deliquescent; they are also for the most part readily soluble in alchol even in the cold. They are most readily obtained by evaporating their aqueous solutions at 212°, and not by adding an acid to the etherial solution of the base; as in the latter case the presence of even a minute proportion of water causes them to precipitate in the form of a semifluid mass. Picoline forms a number of acid salts, in which respect it differs from aniline. Its salts are less readily decomposed in the air than the corresponding aniline compounds, but they do eventually become brown, although without presenting any of the rose red colour which the latter salts assume.

Sulphate of Picoline.—I obtained this salt by supersaturating sulphuric acid with picoline. The solution obtained was perfectly colourless, and when evaporated in the water-bath, it evolved picoline in abundance, and formed a thick oily fluid, which, on cooling, concreted into a tough mass of transparent and colourless crystals, apparently of a tabular form. Exposed to the air, it deliquesces rapidly into a transparent and colourless oil, which, after a time, acquires a slight brownish colour. It is insoluble in ether, but readily in alcohol, both hot and cold. It is not deposited in crystals by allowing the hot alcoholic solution to cool. I analysed this salt by evaporating to dryness in the water-bath, in a weighed platinum crucible, and allowing it to cool under an exsiccator. It was then rapidly weighed, dissolved in water, and precipitated by chloride of barium :—

4·364 grains of sulphate of picoline gave
5·230 ... sulphate of baryta = 41·20 per cent. of anhydrous sulphuric acid.

This result corresponds with the formula $C_{12} H_7 N + 2 H O, S O_3$ as is shewn by the following calculation :—

			Theory.	Experiment.
2 Eq.	Sulphuric acid	1000·0	41·84	41·20
1 ...	Picoline	1164·5	48·74	...
2 ...	Water	225·0	9.42	...
		2389·5	100·00	

The sulphate of aniline dried at 212° has a different constitution, it gives 28·67 per cent. of sulphuric acid, which corresponds to the formula $C_{12} H_7 N, H O, S O_3$.

Oxalate of Picoline.—This salt is obtained by mixing oxalic acid and picoline in excess, and evaporating the solution over quick-lime. When the solution is reduced to a very small bulk, it is deposited in the form of short prisms radiating from a centre; and on further evaporation, the whole concretes into a solid mass. The crystals evolve the odour of picoline in the air; they are highly soluble in water and alcohol, both absolute and hydrated. When heated to 212° it fuses and evolves abundance of picoline vapours, and on cooling it forms a thick fluid which slowly deposits crystals in the form of fine needles. These are probably an acid salt. I did not obtain the oxalate in a state of sufficient purity for analysis.

Nitrate of Picoline is obtained in a white crystalline mass, when a mixture of picoline and dilute nitric acid is evaporated to dryness at a moderate heat. At a higher temperature it sublimes in white feathery crystals.

Hydrochlorate of Picoline may be prepared by mixing picoline and hydrochloric acid, and evaporating on the water-bath. On cooling, the thick fluid which remains consolidates into a mass of prismatic crystals. When heated to a high temperature, it sublimes easily, and deposits itself on the sides of the vessel in transparent crystals, which deliquesce rapidly in the air.

Chloride of Platinum and Picoline.—This salt is easily obtained by adding picoline to a solution of bichloride of platinum, containing an excess of hydrochloric acid; it deposits itself immediately, if the solution be concentrated, but when moderately diluted, it makes its appearance only after the lapse of some time. The crystals which are deposited are rather liable to retain an excess of picoline, which renders it advisable to redissolve them in a dilute solution of chloride of platinum with a little hydrochloric acid. From this solution it is deposited pure, on cooling, in the form of fine orange-yellow needles, which can easily be obtained half an inch long even when operating on very small quantities. It is much more soluble both in water and alcohol than the aniline salt, and indeed than the platinum salts of the organic bases generally. It requires only about four times its weight of boiling water for solution.

The crystals of this salt, after washing with alcohol and ether, and drying at 212°, gave the following results of analysis:—

10·032 grains of chloride of platinum and picoline gave
8·862 ... carbonic acid, and
2·760 ... water.

The determination of the platinum, as formerly mentioned, gave in two different trials 32·544 and 32·522 per cent., the mean of which is 32·533. The analysis corresponds with the formula $C_{12} H_7 N, H Cl, Pt Cl_2$.

			Theory.		Experiment.
C_{12}	=	900·0	24·07	.	24·09
H_8	=	100·0	2·67	.	3·05
N	=	177·0	4·73	.	...
Cl_3	=	1330.4	35·59	.	...
Pt	=	1232·0	32·94	.	32·533
		3739·4	100·00		

Chloride of Picoline and Mercury.—When picoline is added to a concentrated solution of bichloride of mercury, a white curdy precipitate immediately falls. If, however, the solution be dilute, it is not precipitated for some time, and then appears in the form of radiated silky needles. It is sparingly soluble in cold water, more readily in hot. It dissolves pretty abundantly in boiling alcohol, and the solution, on cooling, deposits it, sometimes in prismatic, sometimes in feathery crystals. It dissolves readily in dilute hydrochloric acid, with the formation of a peculiar compound which I have not particularly examined. Boiled with water it is decomposed, picoline being evolved, and a white powder being deposited.

In the analysis of this compound I interposed, between the combustion tube and the chloride of calcium apparatus, a small tube in which the mercury and water were condensed, and at the conclusion of the process, a current of dry air, heated to 212°, was drawn through the tube, by means of which the water was conveyed into the chloride of calcium apparatus. The salt was dried simply by exposure to the air, as it loses picoline when heated; when analysed it still smelt of picoline, which accounts for the excess of carbon obtained.

The following are the results of the analysis:—

10.962 grains chloride of mercury and picoline gave
8·245 ... carbonic acid,
2·168 ... water.

This corresponds to the formula $C_{12} H_7 N + Hg Cl_2$, which gives the following results:—

			Theory.		Experiment.
C_{12}	=	900·0	19·63	.	20·51
H_7	=	87·5	1·90	.	2·19
N	=	177·0	3·86	.	...
Cl_2	=	887·0	19·35	.	...
Hg	=	2531·6	55·26	.	...
		4583·1	100·00		

This salt differs in constitution from the aniline salt, which is represented by the formula $2 (C_{12} H_7 N) + 3 Hg Cl_2$; it tallies, however, perfectly with the compound of chinoline and bichloride of mercury, which is $C_{18} H_8 N + Hg Cl_2$.

I have not particularly examined the other compounds of picoline.

Products of the Decomposition of Picoline.

The small quantity of picoline at my disposal has hitherto prevented my examining particularly the products of its decomposition, a branch of the subject which presents numerous points of interest. Such results, however, as I have obtained, indicate a striking difference between the products afforded by it and aniline.

When treated with nitric acid of specific gravity 1·5, picoline is immediately dissolved, but without communicating to the fluid the fine indigo-blue colour which aniline produces under similar circumstances. On the application of heat there is produced an extremely slow evolution of nitrous fumes, which contrasts strikingly with the tumultuous action which aniline produces. After very long-continued treatment with nitric acid, the fluid was evaporated to a very small bulk, when it deposited large crystals in the form of rhomboidal tables. These crystals, on being treated with potass, evolved picoline unchanged. The potass solution was red, but it contained no carbazotic acid, at least no carbazotate of potass was deposited on evaporation.

An excess of bromine water added to picoline causes an immediate and abundant precipitate of a reddish colour, which, on standing during the night, deposited itself in the form of a transparent reddish oil. This substance is destitute of basic properties, and is readily soluble in alcohol and ether, but not in water. Aniline, when treated in the same manner, gives, as is well known, the bromaniloid of Fritsche, which is solid, and crystallises in silky needles, fusible at 232°. It seems probable that the oily fluid obtained from picoline may possess a constitution similar to that of bromaniloid, in which case it would have the formula $C_{12} (H_4 Br_3) N$, and would receive the name of bromopicoloid. I had not enough of it for analysis.

The action of chlorine on picoline is remarkably analogous to that which it produces on aniline. When passed into anhydrous picoline it is rapidly absorbed, and colourless crystals, apparently of hydrochlorate of picoline, are deposited. In a short time, however, the fluid becomes dark brown, and is finally converted into a resin. This resin was mixed with water, and a current of chlorine passed through it for some hours. The fluid was then introduced into a retort, and distilled, a crystalline substance, passed over along with the water, and after all the water had passed, another substance made its appearance, while a large quantity of carbon was left in the retort. The quantity in which I obtained these substances was far too small to admit of their particular examination, but it appeared

to me that the odour of the latter substance was different from that of c[illegible]ro-phenesic acid, which is produced by the action of chlorine on aniline.

The preceding investigation is sufficient to establish the identity, in co[illegible]u-tion and difference, in properties of picoline and aniline. These substanc[illegible]re then isomeric, in the strict sense of the term, possessing the same comp[illegible]on per cent., and the same atomic weight.

Although isomerism has been recognised in a great variety of different c[illegible]es of compounds, I believe the present to be the first instance in which it ha[illegible]en satisfactorily proved among organic bases. Two instances, indeed, have[illegible]en previously described, but in neither can the evidence be considered abso[illegible]ly conclusive. One of these cases is that of two bases discovered by PELLETI[illegible]nd COUERBE* in the husks of the Cocculus Indicus, to which they have giv[illegible]he names of Menispermin and Paramenispermin. The characters which they[illegible]ve assigned to these substances are sufficiently distinct, but their analyses o[illegible]th lead to the formula $C_{18} H_{12} N O_2$. This result, however, is unsupported by a[illegible]e-termination of their atomic weights, without which the isomerism can[illegible]be admitted as proved. The other instance is that of bebeerine, which, acc[illegible]ng to the analysis of Dr D. MACLAGAN,† is isomeric with morphia, both being [illegible]e-sented by the formula $C_{35} H_{20} N O_6$; and as this result is supported by the an[illegible]is of the platinum compound, the probability of their isomerism is much highe[illegible]n in the former case. Unfortunately, however, another source of fallacy [illegible]rs into the question in the amorphous condition of bebeerine, which renders [illegible]n-possible to determine with certainty its freedom from impurity; even the c[illegible]i-tution of morphia, by far the most definite of the two substances, can scarc[illegible]be considered as fixed, GERHARDT, for instance, representing it by the fo[illegible]la $C_{36} H_{19} N O_6$, and not by that formerly given.

With aniline and picoline, however, these uncertainties disappear. [illegible]th substances are possessed of definite boiling-points widely different fro[illegible]ne another, and of all the other physical characters of pure substances. Th[illegible]w-ness of their atomic weight also precludes any possibility of doubt regardi[illegible]he true formula, and enables us to speak with certainty as to the identity o[illegible]ir constitution. The isomerism of these substances is, moreover, of much [illegible]er interest in a theoretical point of view. Menispermin and morphia are i[illegible]ed substances, entirely unconnected, in constitution or general relations, wit[illegible]ny other substance. Aniline, on the other hand, is a member of one of the[illegible]st extensive, widely distributed, and interesting groups of substances, with [illegible]ch the recent discoveries of organic chemistry have made us acquainted, the [illegible]go Salicyl and Benzoil series. The members of this larger group already pre[illegible] a variety of instances both of isomeric and polymeric compounds, a few of w[illegible] I

* Annales de Chimie et de Physique, vol. liv.

† Proceedings of the Royal Society of Edinburgh, No. 26.

e here brought together in the form of a table, which does not pretend to any ntific arrangement, its sole object being to point out the remarkable relations niline and picoline to the group.

Indigogene,	$C_{16} H_6 NO_2$	Indine.
Indigo,	$C_{16} H_5 NO_2$	...
Isatine,	$C_{16} H_5 NO_4$	...
Anthranilic acid,	$C_{14} H_7 NO_4$	...
Salicylic acid.	$C_{14} H_6 O_6$	?*
Nitrosalicylic acid,	$C_{14} H_5 (NO_4) O_6$	...
Benzoic acid,	$C_{14} H_6 O_4$	Salicylous acid.
Nitrobenzoic acid,	$C_{14} H_5 (NO_4) O_4$	Nitrosalicylous acid.
Chlorobenzoic acid,	$C_{14} H_5 ClO_4$	Chlorosalicylous acid.
Hydruret of benzoil,	$C_{14} H_6 O_2$	Benzoine.
Benzonitril,	$C_{14} H_5 N$	Azotide of Benzoil.
Stilbene,	$C_{14} H_6$	...
Phenol,	$C_{12} H_6 O_2$	...
Aniline,	$C_{12} H_7 N$	Picoline.
Tribromaniline,	$C_{12} H_4 Br_3 N$	Tribromopicoline ?
Benzin,	$C_{12} H_6$	?
Nitrobenzid,	$C_{12} H_5 (NO_4)$	...

The facility with which aniline can be obtained by the decomposition of dif- nt members of this group, renders it by no means impossible to anticipate the ficial production of picoline also.

As we can start from benzoic acid, and convert it into benzin, benzin into obenzid, and that finally into aniline by the action of sulphuretted hydrogen, eems by no means improbable that salicylous acid, the isomeric of benzoic , may be made to undergo a similar series of changes, the final result of ch would be the formation either of picoline, or of some other compound eric with it and aniline. In order to subject this hypothesis to the test of eriment, I mixed salicylous acid with equal weights of slaked lime and caustic yta, and distilled in the oil bath, with the view of obtaining a substance which ld be isomeric with benzin. The greater part of the salicylous acid, how- r, passed over unchanged; but by agitating with solution of potass, there was undissolved an excessively minute quantity of a solid crystalline substance. ding this mode of operating unsuccessful, I passed salicylous acid over spongy inum heated to a very low red heat in a glass-tube. A dark viscid oily fluid sed over into the recipient, of which the greater quantity dissolved in caustic ss, but left behind a larger quantity of the solid substance than was yielded he first experiment. By distillation with water this substance passed into receiver in the form of oily drops, which solidified on cooling, and formed a stalline mass in which minute needles could be detected. It had a peculiar sant smell which resembled that of benzin; but the quantity which I ob- ed was much too minute to admit of its analysis, or of any attempt to con- t it into picoline.

* GERHARDT has observed (Precis de Chimie Organique, tom. ii., p. 21), that benzoic acid, when with hydrate of potass, evolves hydrogen, and gives the potass salt of a new acid. This may pos- be isomeric with salicylic acid.

POSTSCRIPT.

Although the analogy existing between picoline and the other oleaginous bases is perfectly sufficient to warrant the assumption of the absence of oxygen in that substance, I have thought it advisable to append here an experimental determination of the nitrogen. As the volatile bases cannot be readily analysed by VARRENTRAP and WILL'S method, I made a combustion of the platinum salt, and determined the proportion by volume of the carbonic acid and nitrogen in four tubes, which gave the following results :—

I.	94	volumes gave	8·	nitrogen.
II.	240	...	18·	...
III.	84	...	6·5	...
IV.	421	...	35·	...
	839		67·5	

These results give the gases in the proportion of $11\frac{1}{2}$ to 1 ; in other words, they shew a slight excess over the theoretical result, according to which they should be in the proportion of 12 to 1. They confirm perfectly, however, the absence of oxygen.

XV.—*Results of the Makerstoun Observations, No. II. On the Relation of the Variations of the Vertical Component of the Earth's Magnetic Intensity to the Solar and Lunar Periods.* With a Plate. By J. ALLAN BROUN, Esq., *Director of* General Sir T. M. BRISBANE'S *Magnetical and Meteorological Observatory. Communicated by* General Sir T. M. BRISBANE, Bart.

(Read 20th April 1846.)

1. The following results are deduced from the observations of the balance or vertical force magnetometer, which consists of a magnetic needle, balanced horizontally, and resting, by a knife-edged axle, on agate planes. Much doubt has been entertained as to this instrument's capability of shewing changes of moderate nicety, and it has been considered altogether unavailable for changes of long period:* it has been shewn (Vol. XVI., p. 67), that there are several difficulties in the way of an accurate interpretation of the observations, independent of the instrumental capacity. If it be added, that disturbances seem to affect the daily means of the vertical component,† in a more serious way than they do those of the horizontal component, it will be seen that there are a series of difficulties, which tend to render good and consistent results from the balance magnetometer nearly unattainable. It will be judged afterwards how far these difficulties have been overcome in the present instance.

2. The changes of the vertical component are, in general, very gradual and regular; even during considerable disturbances the balance needle moves gradually to an extreme position, remaining there, with moderate fluctuations, for a considerable period, and afterwards returning slowly to its mean (or nearly mean) position; the bifilar magnet, on the contrary, moves with considerable rapidity from one extreme position to another. This difference in the mode of disturbance, I do not conceive due to a want of sensibility in the balance needle, but rather to a difference in the nature of the disturbances of the dip, and of the total intensity.

3. The changes of the vertical component, then, during disturbances, will evidently have little or no effect on the *regularity* of the diurnal curves, although the periods of maxima and minima may be affected; it will be different for the daily means, for, as the disturbances will, in general, be almost wholly negative

* Revised instructions by a Committee of the Royal Society of London, p. 37.

† I shall generally use the terms vertical or horizontal component, instead of vertical or horizontal force; the latter are not at all expressive where the changes may be altogether due to variations of dip.

or positive, there will be sudden and considerable daily decrements or increments of the vertical component. If the effect of these disturbances be equally positive and negative, it may be expected that, in the mean of a sufficient number of observations, they will destroy each other, or, if almost wholly negative, that they will be distributed equally over any period for which the law is desired. If, however, we expect consistent and regular results, from short series of observations, especially when the range of the variations for the resulting law is small, it is evidently desirable to eliminate disturbances as far as possible. To do so completely, requires a knowledge of the observations which should be classed as disturbed, a matter of difficult attainment, as disturbances have various magnitudes, and we have to distinguish between variations which are periodic, and those which are not so; the latter constituting what are distinctively termed disturbances. Without doubt, disturbances seem to obey an *irregular* periodic law, that is to say, they occur more frequently at certain hours than at others; but there are whole months in which they are not at all apparent, and in which, each day's observations represent equally, in range and periods of maxima and minima, the diurnal law; thus, in the months of June and July 1844, each day's observations of the horizontal component when projected, exhibit, with a few slight exceptions, a complete representation of the monthly curve, affected, however, to some extent by the varying hour angle of the moon.

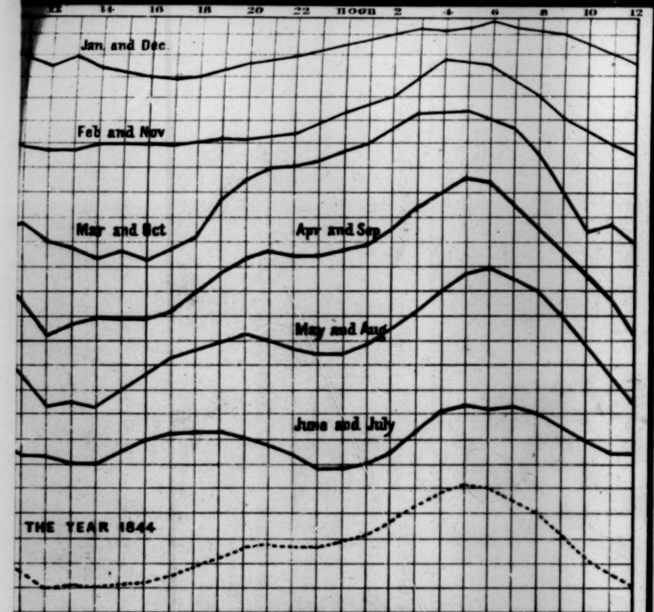
4. As it is only the larger disturbances which affect the periodic result to a marked extent, the complete elimination of disturbances is fortunately not of much moment; the methods which I have adopted for the elimination of these will be stated afterwards; it will be found, however, that I have depended as little as possible on these eliminations.

5. The results as yet obtained for the variations of the vertical component of the earth's magnetic intensity are confined, as far as I am aware, to determinations of the mean period of diurnal maxima and minima. These may be obtained from a good instrument, with moderate exactness, in spite of considerable inaccuracy in the temperature correction, as the mean diurnal variation of temperature, in well closed rooms and boxes, will not exceed a few degrees of Fahrenheit. The Toronto observations for 1841 and 1842, give consistent results, indicating

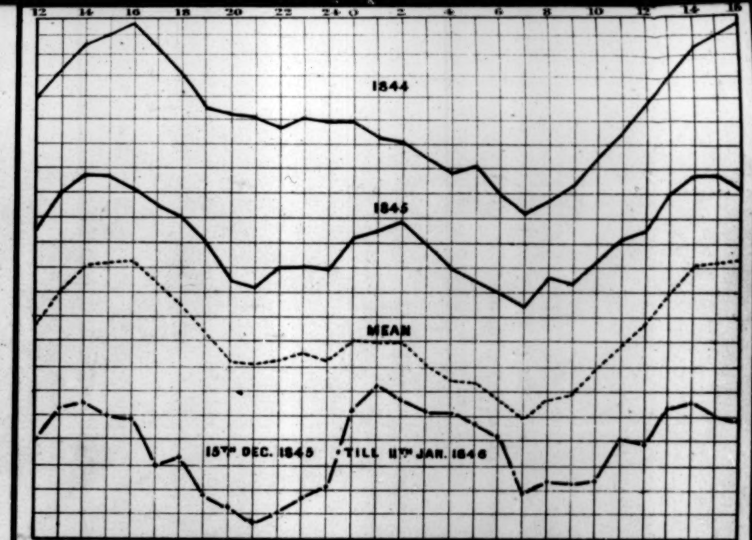
A principal maximum at 6^h Toronto mean time.
A principal minimum at 14^h ... ...
A secondary maximum at 20^h ... ...
A secondary minimum at 22^h ... ...

There are only three months of the twenty-four indicating a single maximum and a single minimum, namely, the months of November 1842 and September of each


Jan. and Dec.
noon

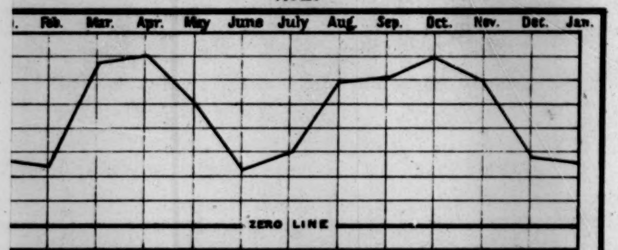


DIURNAL PERIODS IN 1844.



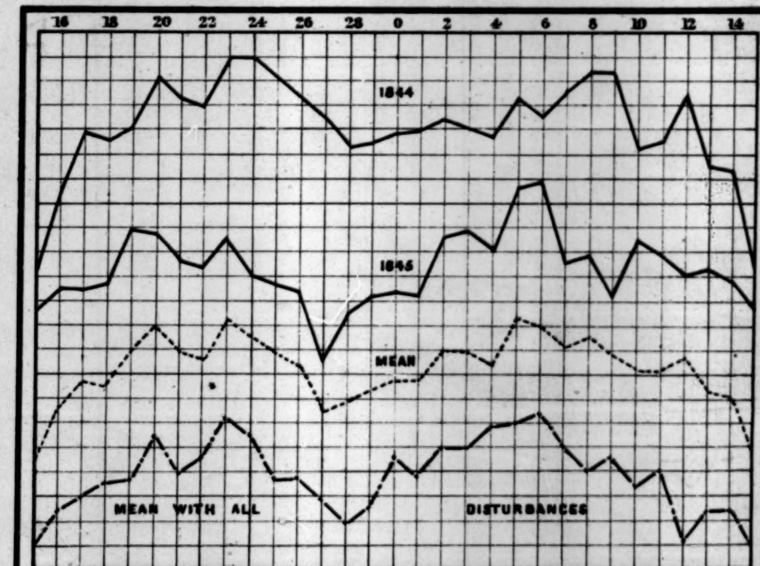
ABSCISSÆ = $\frac{24}{25}$ OF THE MOON'S HOUR ANGLE.

No. 2.



MEAN DIURNAL RANGES IN 1844.

No. 4.



ABSCISSÆ = DAYS OF THE MOON'S AGE.

Schenck Lith.r Edin.r

CURVES

To illustrate the Changes

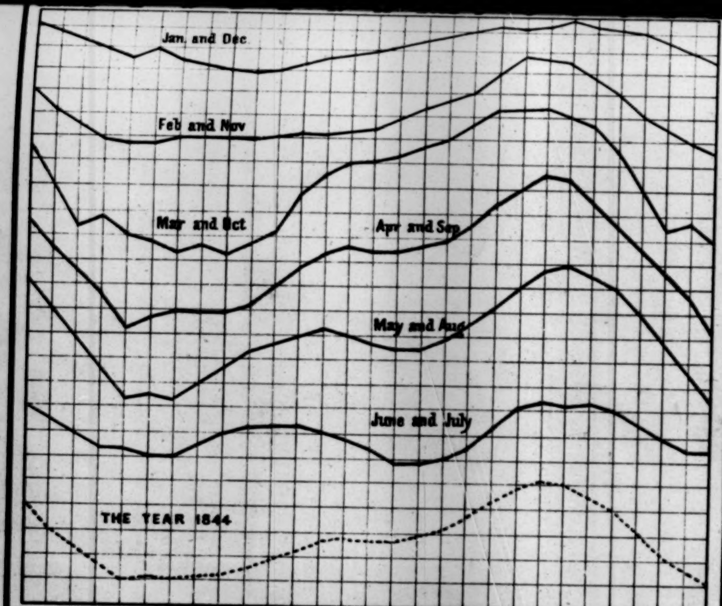
of the

VERTICAL COMPONENT OF THE MAGNETIC FORCE

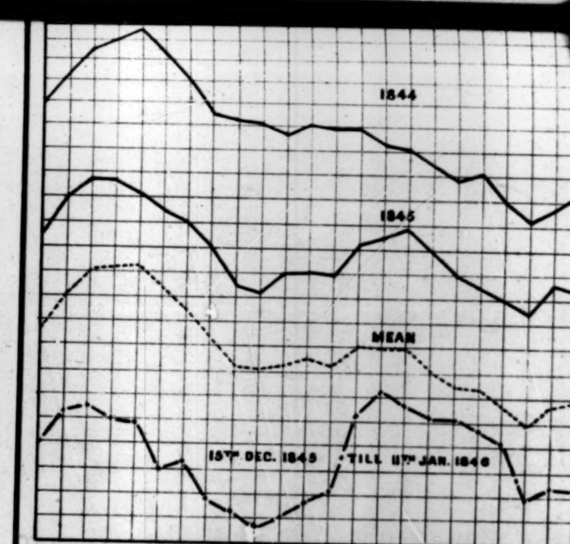
at

MAKERSTOUN.


Jan. and Dec.

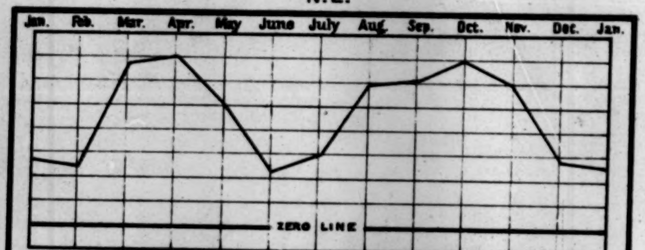


DIURNAL PERIODS IN 1844.



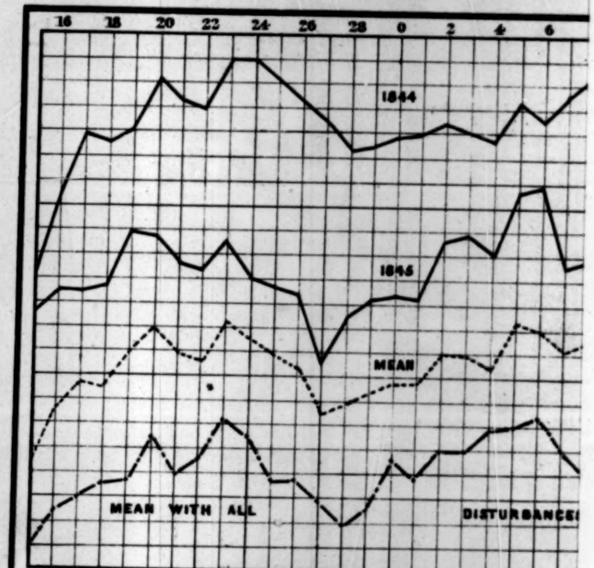
ABSCISSÆ = $\frac{24}{15}$ OF THE MOON'S HOUR ANGLE

N°.2.



MEAN DIURNAL RANGES IN 1844.

N°.4.



ABSCISSÆ = DAYS OF THE MOON'S AGE

F. Schenck Lith.r Edin.r

CURVES

To Illustrate the Changes

of the

VERTICAL COMPONENT OF THE MAGNETIC FORCE

at

year; the diurnal range is greatest in summer, and least in winter.* These results will be found to differ considerably from the following, excepting in the periods for the whole year.

6. The mean of the vertical component for the year 1844, at Makerstoun, was least at 14^h (Makerstoun mean time); it increased gradually, from that time, till nearly 21^h; it then diminished slightly till nearly 23^h; after which it increased rather rapidly till $5^h\ 30^m$; it then diminished with much rapidity till midnight, increasing slightly from 12^h to 13^h, and diminishing slightly again to the minimum at 14^h; or the vertical component has

The principal minimum.........	at $14^h\ 10^m$	Makerstoun mean time.
A secondary maximum	at $20^h\ 50^m$	... ...
A secondary minimum	at $22^h\ 50^m$	... ...
The principal maximum	at $5^h\ 30^m$	... ...
Perhaps a third minimum......	at $12^h\ 10_m$	... ...
And a third maximum	at $13^h\ 10^m$	... ...

The slight inflexion at 13^h (See Mean Curve, No. 1., Plate VI.) would not have been noted as a maximum, but simply as a slight irregularity probably due to disturbances, were it not for the evidence which, with one exception, every month in the year gave to the same effect. If, however, it be considered simply as an irregularity (it will be the only one in the whole period), $13^h\ 10^m$ must be taken as the actual period of the principal minimum, the vertical component having nearly equal values at $12^h\ 10^m$ and $14^h\ 10^m$.

7. The form of the diurnal curve, and the periods and number of maxima and minima vary throughout the year. Having found that the diurnal curves, for each of the two months symmetrically placed with regard to the solstices, were nearly identical in periods and range, only the mean curves for each couple have been projected (Curves, No. 1, Plate VI.), namely, the means for the months of

January and December.	April and September.
February and November.	May and August,
March and October.	June and July.

8. The transitions of the periods of maxima and minima throughout these months is curious; the principal maximum occurs about 6^h at the solstices, near 4^h at the equinoxes, and about 5^h at the intermediate periods. The principal minimum occurs about 17^h at the winter solstice, about midnight at the equinoxes, and about noon at the summer solstice. In January and December there

* Toronto Observations for 1840, 1841, and 1842. Abstracts, &c., p. lx. The Toronto observations were made every two hours, and the Makerstoun observations every hour.

is no minimum visible near noon; in February and November an inflexion occurs there; in March and October there is a marked tendency to a minimum at that time; there is a well-marked minimum near noon in April and September; a strongly marked one about the same time in May and August; and the principal minimum occurs in June and July near mid-day. The same progression holds for the maximum, which occurs near 20^h, excepting that it never becomes the principal maximum, though in June it differs very little from it. The inferior maximum and minimum are best marked near the equinoxes.

The following table will exhibit the transitions of the periods with more distinctness; the negative sign (−) is placed before the hour of the principal minimum, and the positive sign (+) before the hour of the principal maximum.

TABLE of the Hours, Makerstoun mean solar time (astronomical reckoning), of the Maxima and Minima of the vertical component of Magnetic Intensity for 1844.

Months.	Minimum.		Maximum.		Minimum.		Maximum.		Minimum.		Maximum.	
	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.	h.	m.
January and December,	−17	10	...		...		+6	10	12	10	13	10
February and November,	−?16	40*	...		...		+5	0	12	40	14	40
March and October,	−16	10	(22)	?	(0)	?	+4	10	14	10	15	10
April and September,	15	10	21	10	23	40	+5	20	−12	10	14	20
May and August,	−14	10	20	10	23	40	+5	50	12	10	13	10
June and July,	13	30	18	50	−23	40	+6	0	11	30	12	0

It will be observed that the hours of the first minimum and maximum in this table occupy a year in completing their transitions, that the hour of the second minimum in the table is constant, and that the hours of the second maximum, and of the last minimum and maximum in the table, complete their transitions in six months.

9. Neglecting the inferior maximum and minimum, it will be remarked that the period of duplication in the forms of the curves differs completely from that for the horizontal component. For the latter, it is summer in which the diurnal curve becomes single, and winter in which it becomes double. It is the secondary or morning *maximum* of the horizontal component which becomes the principal maximum in *winter;* it is the secondary or noon *minimum* of the vertical com-

* The principal minimum in the mean for the months of February and November actually occurs at 12^h 40^m. This is due to an apparent irregularity in the month of November, that month being the only one which does not shew the inferior maximum near midnight.

ponent which becomes the principal minimum in *summer*. For the horizontal component the 22^h minimum is *the* minimum throughout the year; for the vertical component the 5^h maximum is *the* maximum throughout the year.

10. Perhaps the most curious fact in connection with the vertical component is that of the annual variation of the diurnal range. It has always been imagined, I believe, that the diurnal range of all the magnetical elements increased from winter to summer. This has been shewn to be the case for the horizontal component (No. I., 6*), and will afterwards be shewn to be true for the magnetic declination. It is not so, however, for the vertical component, as may be seen by a glance at the six diurnal curves projected (Curves, No. 1, Plate VI.). The transition in form and range is evidently worst exhibited by the mean for February and November; this, however, and other irregularities, may probably be due to disturbance. The elimination of these (which I have not at present attempted), or the observations of other years, must decide this. When the range for each month is projected (Curve No. 2, Plate VI.), it is at once evident that the diurnal range is least at the solstices, and greatest at the equinoxes. The mean of the ranges for January and December, and also the mean for June and July (the solstitial months), is about 0·00028, the whole vertical component being unity, while the mean for each of the couples of equinoctial months, namely, of March and April, and of September and October, is about 0·00068.

11. It might have been expected that this curious variation of the ranges would shew itself more or less in the ranges for the horizontal component. If we refer to No. I. of these results (6), page 101 of this volume, and to the projected curves, No. 2 of that series (Plate III.), we shall find this actually the case, although in that place it was supposed that the variations in the regular increase of the ranges might be due to disturbances. These facts seem to point to a difference in the modes and causes of increase of the diurnal range for the magnetic dip, and for the total magnetic intensity; the diurnal ranges of the latter seeming to obey a law which is equally related to the two solstices, and also to the two equinoxes, a circumstance in favour of the annual period previously announced (No. I., 10).

12. The mean values of the vertical component, at 21^h and 0^h, are nearly equal to the mean for the year, but no single hour, as for the horizontal component (No. I, 8), indicates the mean for each month.

13. Proceeding in the order of No. I., I should now consider the annual period. The results for the different years are discordant. This, it is my opinion at present, is due to an insufficiency in the temperature correction, which will be found alluded to elsewhere.† The results of the years 1842 and 1843, the latter more strongly, indicate a period similar to that found for the horizontal component, namely, maxima of

* Reference to Results of the Makerstoun Observations, No. I., p. 99 of this volume.

† Introduction to the Makerstoun Observations for 1843.

the vertical component about the solstices and minima about the equinoxes; 1844 and 1845 also indicate a maximum at the winter solstice, but the maximum at the summer solstice is imperfectly shewn, if shewn at all.

14. It is quite evident that a moderate error in the temperature correction may be sufficient to destroy all appearance of an annual period, especially when the range of the temperature may be 30° Fahrenheit, and the range of the component for the annual period may be small. It is, I am inclined to think, due to the existence of a lighted stove in the Observatory in the winters of 1842, and in the beginning of 1843 (by means of which the annual range of temperature was much diminished), that these years give a somewhat distinct indication of the annual period. For many reasons, however, it is to the observations made in the years 1844 and 1845 that I look for a consistent exhibition of the annual period; and to the result for these years I shall return when the insufficiency alluded to has been remedied.

15. The vertical component has diminished considerably since 1841, the yearly rate of diminution becoming less in each year. Something of this apparent change may be due to a loss of magnetism in the balance needle; but it is believed that this is only partially, if at all, the case. There is a curious change in the rate of diminution of the vertical component in the year 1845: in October, November, and December, it was constant or very nearly so; it was remarked of the horizontal component for 1845, that it had increased much less than in the previous year. Does this point to an approaching turning-point for the diminution of the magnetic dip?

16. Similar summations, to those indicated for the horizontal component (No. I., 25), were made for the vertical component, at the varying hour angles of the moon; the larger disturbances were also eliminated similarly, the test number for disturbances being taken more than twice as great in 1845 as in 1844. From a mean of 12 lunations in 1844 (see Curves No. 3, Plate VI.), the maximum of vertical component occurred when the moon had passed the inferior meridian about three hours, the value of the component then diminished considerably till 19^h (counting the moon on the meridian 0^h, and so up to 24^h, when it wants one hour, or, more exactly, $\frac{24}{25}$ of an hour of being on the meridian again; each of the so-called hours having only this value, 19^h corresponds to about $5^h\ 45^m$ before the moon's meridian passage); it diminishes slightly from 19^h till 22^h, when there is a minimum; it then increases slightly till 24^h or 0^h when there is a maximum; after this it diminishes moderately again till 7^h, when the principal minimum occurs; it then increases rapidly to the maximum at 16^h. When the disturbances are not eliminated the maximum occurs at 15^h, the component then diminishes rapidly, with some irregularities, till 2^h, which is the time of the principal minimum; a secondary maximum then occurs at 4^h, and a minimum, differing very little from the other in value, occurs at 6^h or 7^h. A more complete elimination of disturbances, it is conceived, would render the maximum about 0^h still more evident.

17. In 1845 the maximum occurs between 14^h and 15^h, or about 2^h after the moon has passed the inferior meridian, the value of the vertical component then diminishes till 21^h when there is a secondary minimum, it then increases considerably till 2^h; a secondary maximum occurring about $1\frac{3}{4}^h$, it again diminishes till 7^h, when the principal minimum occurs, after which it increases rapidly to the principal maximum. This year's result is little affected by the elimination of disturbances; the curve is rendered somewhat more regular.

18. The results for the two years differ slightly from each other. The principal maximum occurs about an hour later in 1844 than in 1845; the principal minimum occurs at the same time in both years; but the secondary maximum and minimum are not nearly so well defined in 1844 as in 1845. The mean of the two years gives the following periods:—

The principal minimum about 5 hours before the moon's passage of the inferior meridian.
The principal maximum about 3 hours after ....
A secondary minimum about 4 hours before .... superior ....
A secondary maximum about 1 hour after ....

19. These periods are surprisingly near those indicated (No. I., 23), for the horizontal component, the principal maximum and secondary minimum of the vertical component occurring about an hour after those of the horizontal component, while the principal minimum and secondary maximum occur an hour or more before those of the horizontal component; seeming to indicate that these variations belong chiefly to the total intensity.

20. As evidence of the accuracy of the results obtained after eliminating disturbances, nothing perhaps could be more conclusive than the fact, that in those months in which no disturbances occur, the general law is found well marked: it would not be difficult to bring this kind of evidence to bear, both for the horizontal and vertical components. I shall satisfy myself at present by giving the projection of one month's results, namely, the results for the lunation included by December 15. 1845, and January 10. 1846 (Curves No. 3, Plate VI.). From this single month the same, or very nearly the same, periods of maxima and minima are obtained as in the mean for the two years. The maxima have nearly equal values, and so also have the minima; the principal maximum and minimum, however, occur at the times of the secondary maximum and minimum of the mean curve. Such a difference in the values of the maxima might be expected; for, though the moon has every declination in the course of one lunation, it is full only at one of these declinations. It would not have been difficult to have obtained a curve from one month's observations representing the mean curve better.

21. In order to obtain the variations of the vertical component with respect to the moon's age, summations were made similar to those indicated (No. I., 18) for the horizontal component. It should be mentioned (as it should have been before for the horizontal component), that in all the results for monthly periods mean

values for Sundays were used. These were obtained by interpolation; the mean of the three days preceding, and of the three days succeeding, were taken as the means for the Sundays. The want of approximate means for these days would tend to destroy the regularity and distinctness of the results, owing to the variations, due to different causes, which it is desired to eliminate, and that the blank days enter irregularly into the days of the mean monthly periods.

22. The results of these summations (Curves No. 4, Plate VI.) are, that each year indicates maxima of the vertical component near the quadratures, and minima near the syzygies. In order to render the fact more distinct, and the curves somewhat more regular, eleven days* of greatest disturbance were eliminated in each of the years 1844 and 1845, namely, those days on which the mean value of the vertical component was greater than the mean of the previous and succeeding days by more than 26 micrometer divisions (about three times the resulting range). The principal minimum in 1844 occurs at the period of full moon; in 1845 it occurs about three days before the new moon. In the mean of both years the principal minimum occurs at the time of full moon, the secondary minimum about two days before new moon; the maxima occur between these periods, and they are nearly equal. The lowest curve of No. 4 is the projection of the mean for the two years without eliminating disturbances. It differs little in regularity from the other in which the large disturbances are eliminated. The irregularities in these curves may be partially due to the cause of error already stated (17), namely, an insufficiency in the temperature correction.

23. Whether due to the cause just mentioned or not is uncertain, but the results for the relation of the variations of the vertical component to the moon's declination are neither distinct nor consistent for the two years, and the elimination of the days of large disturbance does not improve them. The mean for the two years seems to shew something like the law found for the horizontal component, namely, maxima about the periods of greatest north and south declination; but I do not place any trust at present in this result.

MAKERSTOUN, *April* 13. 1846.

* Each of these days was actually observed (observations were made) at the time as a day of disturbance.

XVI.—*On the Solubility of Fluoride of Calcium in Water, and its relation to the occurrence of Fluorine in Minerals, and in Recent and Fossil Plants and Animals.* By George Wilson, M.D.

(Read April 6. 1846.)

1. *Introductory Remarks.*

The investigation I am about to bring before the Royal Society, was undertaken in consequence of a discussion which took place in the Zoological Society of London in 1843,* in reference to the chemical composition of the bones of the gigantic bird the Dinornis, discovered some time previously in New Zealand. At the meeting in question, the distinguished palæontologist Dr Falconer drew attention to the frequent, if not constant, occurrence of fluoride of calcium in fossil bones, and, as he stated, also in those of mummies; and threw out the suggestion, that the fluoride might shew itself in these animal remains, not as an original ingredient of the bones, or as derived from the matrix in which they were found, but as a product of the transmutation of their phosphate of lime. The idea of such a conversion taking place, is as old at least as the days of Klaproth, who suggested the possibility of phosphoric acid becoming changed into fluoric.† It is commented upon by Fourcroy and Vauquelin,‡ as well as by Gay Lussac,§ as a thing possible but not probable, and which their ignorance of the nature of fluoric acid prevented them from discussing satisfactorily.

The revival of this suggestion by Dr Falconer, at a period when the possibility of the chemical elements undergoing transmutation was occupying the attention of English chemists, and avowedly with a view to shew at least the possibility of such an idea proving true, excited much discussion, and led, I believe, to the researches of Mr Middleton and Dr Daubeny, which I am presently to mention, and of which my own may be considered the sequel. I have to request the forbearance of the reader, whilst, with as much brevity as possible, I refer to the labours of my predecessors in relation to the presence of fluorine in different bodies.

In 1802, Morichini of Rome discovered fluoride of calcium in the molars of a fossil elephant, and was led, in consequence, to search for it in the enamel of recent human teeth, where he also found it.‖ His results were confirmed by Gay Lussac, who experimented along with him,¶ and by Berzelius, who found

* Literary Gazette, Dec. 2. 1843, p. 779.
† Annales de Chimie, tome lvii. (1806), p. 43.
‡ Ibid., p. 44.
§ Ibid., tom. lv., p. 265.
‖ Ibid., p. 258.
¶ Ibid.

the fluoride in the recent bones of man and of the ox; and ascertained the proportion in which it was present in both.* On the authority of these chemists, fluorine was ranked among the constituents of animal bodies. Many excellent observers, however, soon after declared themselves unable to detect that element in recent bones. Among these are FOURCROY, VAUQUELIN,† WOLLASTON, BRANDE, Dr T. THOMSON,‡ GIRARDIN, PREISSER, and REES;§ the last of whom is not content with stating that he found no fluorine in unburied bones, but affirms that no one else can have met with it in them. More recently, Mr MIDDLETON ‖ and Dr DAUBENY ¶ have experienced no difficulty in confirming the original results of MORICHINI and BERZELIUS. An American observer has been equally successful.** Dr GREGORY informs me that he has made many examinations for fluorine in recent bones, and has always found it present in them. My own experience of the subject is to the same effect. I shew the Society glass etched by recent human bones, male, female, and fœtal, which were obtained, without special selection, from the dissecting-room; likewise glass corroded by hydrofluoric acid from the tusk of the recent elephant, and the teeth of the recent hippopotamus, walrus, leopard, and shark.

I shall return, in another section of this paper, to the consideration of the question, how the discrepance in the statements of observers concerning the presence of fluorine in recent bones is to be accounted for. It was the occurrence of that element in fossil bones which gave rise to the discussions concerning its origin, to which I shall have occasion to refer. Fluorine is not a constant ingredient of the animal remains in question, according to FOURCROY and VAUQUELIN, who examined some which contained none. But in the greater number of cases it has been found present, so that GIRARDIN and PREISSER have even proposed to consider its existence in an unknown bone as a proof of the latter not having belonged to man or to any recent organism, but to some "antediluvian animal." ††

It is acknowledged, moreover, that in buried bones, especially in those that are petrified, fluorine is frequently present in larger proportion than in recent ones. Thus LASSAIGNE found fifteen per cent. of fluoride of calcium in the bones of the Anoplotherium;‡‡ MIDDLETON ten per cent. in those of various animals from the Sewalik Hills;§§ GIRARDIN and PREISSER nine per cent. in those of the Lamantin. ‖‖ Mr MIDDLETON, indeed, has endeavoured to shew that the proportion of fluoride of calcium increases according to the period of the entombment of the bone at the rate of $1\frac{1}{2}$ per cent. in a thousand years, and has proposed to estimate

* Annales de Chimie, tom. lxi. (1807), p. 256. † Ibid., 1806, t. lvii., p. 41.
‡ Chemistry of Animal Bodies, p. 236.
§ Guy's Hospital Reports, quoted in Edin. Phil. Journal, vol. xxviii., p. 93.
‖ Chemical Society's Memoirs, vol. ii., p. 135. ¶ Ibid., p. 101.
** Edin. Phil. Journal, vol. xxxix., p. 235. †† Ann. de Chim., t. ix. (1843), p. 381.
‡‡ Quarterly Journal of Geological Society, vol. i., p. 216. §§ Ibid.
‖‖ Ann. de Ch. et Ph., t. ix., p. 375, 1843.

the age of bones, and of the rocks containing them, by the per-centage of fluorine in the former.* This idea, however, is certainly unwarranted. In the bones of five fossil animals, including the Plesiosaurus and Ichthyosaurus, GIRARDIN and PREISSER found from one to two per cent. of fluoride of calcium;† whilst in those of the recent ox, BERZELIUS found nine per cent.‡ In the ancient bones there was thus, instead of a much higher per-centage, seven per cent. less of fluorine than in the recent bones. Many other objections might be made to Mr MIDDLETON's view.

Those who deny the existence of fluorine in recent bones, consider the whole amount of that element found in ancient buried ones, as in some way or other a product of fossilisation. According to those, on the other hand, who affirm the presence of that element in recent organisms, only a portion, at most, of the fluorine found in osseous remains has been added since they ceased to be parts of living animals. It is impossible, however, to separate the two questions. We have no data from which to determine whether or not the bones of an extinct animal contained fluorine during its life, and, if it did, how much was present. It will be sufficient, therefore, if I consider what progress has been made in answering the one question, How does fluoride of calcium come to be present in bones, either recent or fossil?

Three replies have been proposed to this query. *1st*, That of Dr FALCONER, already referred to, which, taking for granted that fossil bones contain more fluorine than they possessed whilst parts of living animals, assumes, or rather suggests as possible, that phosphate of lime has been transmuted into fluoride of calcium.§ *2d*, That of LIEBIG, which, going on exactly an opposite assumption, takes for granted (if I understand him aright) that bones of living antediluvian animals contained the same proportion of fluorine which we find in their fossil remains; and refers its greater abundance in these, either to its having been present in larger quantity in the food of their living possessors, than it is in that of existing animals, or to its having been appropriated to a larger amount from it.‖ The third is that of Mr MIDDLETON, who supposes every bone to possess normally two per cent. of fluoride of calcium, and considers all above that which a fossil bone contains, as added to it whilst buried in the earth, by the infiltration of water containing that salt held in solution by some unknown solvent.

It is unnecessary to discuss the first and second propositions referred to. It is impossible, in the present state of our knowledge, either to prove or to disprove them. The idea of transmutation of a phosphate into a fluoride, was doubtless suggested solely because there seemed no other way of accounting for the accumulation of fluorine, and will be abandoned, if it shall appear that recognized

* Quarterly Journal of Geological Society, vol. i., p. 216.
† Annales de Chimie, 1843, pp. 370-78. ‡ Ibid., t. lxi., p. 257.
§ Literary Gazette, 1843, p. 779.
‖ Chemistry of Agriculture, 3d edition, p. 123.

chemical forces can explain the phenomenon. The great German chemist, also, (whose view may be the true one,) will probably modify his opinion, when he finds that fluoride of calcium is soluble in water.

Mr MIDDLETON'S supposition that all bones contain two per cent. of fluor, is certainly untenable, and so is his belief that bones invariably gain fluorine whilst undergoing fossilisation; but he brought satisfactorily to the test of experiment his view that fluoride of calcium may reach the bones both of living and dead animals through the medium of water. His experiments were not made with aqueous solutions, in which a mere trace of fluoride could at best be expected to be present, but with sedimentary deposits, of natural and artificial origin. "I was led," says he, "to institute a series of experiments on aqueous deposits of different ages, and I found, that, with one exception (a pure but incompact stalactite of carbonate of lime), fluorine exists in all, from the most recent deposit down to the old red sandstone, and that it is present in the older in larger proportion than in the newer beds. I think it is, therefore, beyond a doubt, that it is present in water, though, perhaps, in very minute quantity. *What its solvent may be I know not;* but that it is so held in solution my own experiments have demonstrated; and if they had not, the simple fact that the blood conveys it to the bones, would, I apprehend, sufficiently refute any scepticism on the subject."*

It may justly be questioned, whether the fact of a substance being soluble in a highly complex fluid like blood, would entitle us to infer that it was equally soluble in pure water. But it is singular that Mr MIDDLETON, holding such a view, and after finding fluorine in so many aqueous deposits, should not have endeavoured to dissolve the fluoride of calcium in water. He was, doubtless, prevented from making any trials on the subject by the universal statement of chemists, that the salt in question is quite insoluble in water.

2. *Of the Solubility of Fluoride of Calcium in Water.*

Many substances are spoken of by chemists as insoluble in water which are, nevertheless, known to possess a certain slight solubility in that fluid. But fluoride of calcium has been considered so well entitled to the character of total insolubility, that our most accurate analysts, as BERZELIUS and ROSE, have purposely converted fluorine into this salt in their quantitative determinations of the former, and have washed the latter freely with water, and, as they believed, without its suffering any loss. Their example has been followed by all other analysts, and the fact supplies a better proof than any quotation of individual authors could do, that fluoride of calcium has been considered quite insoluble in water. Relying implicitly on the truth of this belief, I sought for a solvent of fluor-spar which could retain it in union with water, and carry it into the tissues of plants and

* Quarterly Journal of Geology, vol. i., p. 215. Mr MIDDLETON'S other papers on fluorine are in the Chemical Society's Memoirs, vol. ii., p. 134; and in the London Phil. Mag., No. 164, p. 14.

animals. The frequent association of phosphate of lime and fluoride of calcium in minerals, naturally suggested that whatever substance enabled water to become charged with the one salt, would cause it to dissolve the other. Carbonic acid is known to be one agent which confers upon water the power of taking up phosphate of lime; it seemed worth while, therefore, to try whether it would cause it to dissolve fluoride of calcium as it does so many other lime-salts. I was not aware that DANA the American mineralogist,* and Professor GRAHAM of London,† had anticipated me in this idea, or I should probably not have performed any experiments on the subject. In ignorance of their views, the following trials were made. A portion of pale green crystallised fluor-spar was reduced to fine powder and digested for some hours in warm nitromuriatic acid, so as to remove any carbonate of lime, metallic oxides, or other foreign matters, which might be present. It was then washed on a filter, dried, and suspended in pure distilled water, through which a current of carbonic acid was passed for two hours. At the end of this period the liquid was filtered through paper, and tested for lime by oxalate of ammonia. A cloudiness was soon occasioned, and speedily a white precipitate. On evaporating the liquid to dryness, a greyish-white residue was left which gave off sharp acid fumes when moistened with oil of vitriol. When this residue was warmed with Nordhausen sulphuric acid in a platina crucible covered by glass, the latter was deeply corroded in a few minutes. The process was repeated many times, and always with the same result. I shew the Society squares of glass which were etched in this way; the engraved words having been traced through wax, as in the ordinary method of testing for hydrofluoric acid. The experiments referred to were made in January last, and were supposed to justify the idea which led to their trial, namely, that carbonic acid was the agent which enabled water to dissolve fluor-spar.

If carbonic acid, however, had been essential to the retention of fluor in solution, the expulsion of that gas, by warming the liquid, should have been followed by the deposition of the fluoride. I was struck, however, by observing that the solution could be raised to the boiling-point, without any troubling or opalescence appearing, and that no precipitate shewed itself after protracted ebullition. It was manifest that water was able of itself to retain in solution the fluoride if once dissolved in it; and highly probable that it would prove equally sufficient to commence the solution of the lime-salt. The experiment was accordingly tried of suspending fluoride of calcium in cold distilled water, and shaking it occasionally in a stoppered bottle for two hours. The liquid, after filtration, shewed lime with oxalate of ammonia as readily as the carbonic acid solution had done, and left, after evaporation, a residue which gave, with oil of vitriol, acid vapours etching

* Edin. Phil. Jour., vol. xxxix., p. 255.

† Note to Mr MIDDLETON's paper, Quart. Jour. Geol. Soc., vol. i., p. 216.

glass. Distilled water was then boiled upon powdered fluor-spar and filtered whilst hot. It precipitated oxalate of ammonia instantaneously; and deposited, after cooling, a small quantity of a white precipitate, which answered to the tests of lime, and, when moistened with strong oil of vitriol, gave off an acid which corroded glass. The supernatant liquid likewise precipitated oxalate of ammonia, but more slowly, and yielded, on evaporation, a residue identical in characters with the deposit from the hot aqueous solution. When the deposit or residue was mixed with pounded glass and oil of vitriol, and heated in a flask, a gas was given off which deposited gelatinous silica when passed through water, and had all the characters of fluosilicic acid. It was manifest from these trials, that water can dissolve fluoride of calcium; and that it is more soluble in boiling than in cold water.

The experiments I have mentioned are of so simple and decisive a kind, that the conclusion they warrant cannot be evaded. That no error might arise from impurity of material, many of them were made with water twice distilled, and ascertained to be quite free from foreign matter. On the other hand, specimens of fluor-spar were obtained from different cabinets; some massive; the greater number well crystallised. The fluor was finely powdered, and thereafter, in the greater number of cases, digested in warm aqua regia, washed and dried. The only foreign body likely to be present, which could escape removal by this treatment, is silica, a substance which would lessen rather than increase the solubility of the fluor. Lest, moreover, the agents employed to purify the fluoride of calcium should be supposed to have conferred on it a solubility which it did not originally possess, other trials were made with native crystals, which, without preliminary treatment, were reduced to powder and boiled with distilled water. In every case solutions were obtained, which, when cooled, yielded a deposit, or, when evaporated, a residue, which gave off hydrofluoric acid when moistened with oil of vitriol, and left sulphate of lime.

The pieces of etched glass which I shew the Society were corroded by hydrofluoric acid obtained from the fluoride of calcium previously in solution in water. They will be observed to be as deeply *bitten in* as if undissolved fluor-spar had been made use of. Four liquid ounces of the cold aqueous solution will be found to leave sufficient residue to etch glass permanently. The residue from the same amount of solution made at 212° Fahr. will act still more decisively.

The solution of fluoride of calcium in water at 60° is colourless, transparent, tasteless, and precipitates oxalate of ammonia. Chloride of barium and nitrate of baryta occasion a white precipitate. These reagents act more readily with the solution at 212°.

The only one of these reactions I have yet found time to examine with any attention is that of the salts of barium. The precipitate they occasion yields hydrofluoric acid abundantly, when treated with oil of vitriol. I have not ascer-

tained whether it is simply a fluoride of barium, as it is likely to be, when nitrate of baryta is employed; or a double fluoride and chloride of barium, as it may be, when the latter is the precipitant. BERZELIUS has described such a salt. But I have frequently availed myself of the fact that barium forms a sparingly soluble compound with fluorine, in seeking for the latter in liquids. They are often most conveniently tested for that substance by precipitating them by a salt of baryta, and testing the precipitate for hydrofluoric acid. This reaction, moreover, has an important relation to qualitative chemical analysis, inasmuch as it throws an unsuspected difficulty in the way of distinguishing dissolved sulphates from fluorides. The barytic precipitate, with solution of fluoride of calcium, is soluble in excess of nitric and hydrochloric acids, but it requires a much larger addition of these to redissolve it, than the carbonate, borate, or phosphate of baryta does. A fluoride, therefore, may readily be mistaken for a sulphate, or a mixture of both for only the latter. This mistake must have been frequently made in analysing mineral waters, where fluorine is certainly more abundant than has hitherto been suspected. When fresh analyses of these bodies shall be made, I have little doubt that where fluorine is met with, as Mr MIDDLETON has already discovered it in the pipe-water of London, and I have detected it in one of the wells of Edinburgh, it is the sulphates that will be found to have been over-estimated, at the expense of whatever proportion of fluorine was also present.

The fact of the solubility of the fluoride of calcium in water introduces an insurmountable objection to the present method of estimating fluorine quantitatively. It accounts in part for the discrepance between the result obtained, when fluorine has been estimated by the loss which a substance containing it sustained when heated with sulphuric acid, as contrasted with that which has been procured when the hydrofluoric acid evolved was condensed in ammonia, and precipitated by solution of chloride of calcium. Dr DAUBENY, for example, mentions that phosphorite from Estremadura, yielded, according to the first method, fifteen per cent. of fluoride of calcium, according to the second, not nine per cent.* Part of the difference was doubtless owing to the difficulty with which fluor-spar is made to abandon all its fluorine when distilled with oil of vitriol, in consequence of the pasty condition of the sulphate of lime which is formed. But when we find Dr DAUBENY mentioning, that he subjected the precipitated fluoride of calcium to "repeated washings with water," in order to remove any accompanying sulphate of lime, we may well suspect that fluoride of calcium was also washed away.

I regret that I cannot yet announce the proportion of fluor-spar which water dissolves. Owing to the corroding action which the solution occasionally exerts on glass, I thought it unadvisable to employ vessels of that material, or of porce-

* Chemical Society's Memoirs, vol. ii., p. 98.

lain. I endeavoured to substitute for these, silver basins, but found it impossible to prevent them gaining weight from the sulphuretted hydrogen constantly present in an analytical laboratory. Through the liberality of Dr GREGORY I have recently obtained a platina basin of much larger dimensions than the resources of my own laboratory afforded, and by means of it I shall be able to announce the proportion of fluor-spar taken up by water. Meanwhile, we cannot doubt that the proportion of fluorine has hitherto been estimated too low in most of the substances ascertained to contain it. In some cases the error must have been considerable.

I had hoped that the barium salt of fluorine would prove suitable for the quantitative estimation of the latter. It certainly would be much better than the fluoride of calcium, but according to BERZELIUS it possesses a certain though slight solubility in water. Fluoride of barium must accordingly be rejected also, unless no better compound for estimating fluorine quantitatively can be discovered. I may remark, in passing, that the fact that fluoride of barium is soluble in water might have led to the discovery that the similar salt of calcium was so likewise. The salts of barium, as a class, are much more insoluble than those of calcium. If, therefore, the barium compound of a salt-radical be soluble in water, the calcium salt of the same radical should, *a fortiori*, be still more so.

The observation of the previously unsuspected solubility of fluor-spar in water, promised to throw light on some interesting problems connected with geology, mineralogy, and physiology. I was induced, in consequence, to make a series of researches in reference to these points, the results of which I shall now briefly state.

Many of the investigations were very tedious, and I take this opportunity of expressing my obligations to two of my pupils, Mr HENRY C. BRIGGS and Mr HENRY WILLIAM STANSFELD, for the cordial and untiring assiduity with which they aided me in my researches. To my assistant, Mr DAVID FORBES, I have likewise been greatly indebted for the most active co-operation throughout the inquiry.

3. *Of the presence of Fluorine in Well, River, and Sea Water.*

It was impossible to doubt, after the facts I had observed in the laboratory, that fluorine must be no infrequent constituent of well and river as well as of sea water. BERZELIUS mentions that fluoride of calcium has been found in the waters of Carlsbad.* Dr CHRISTISON has pointed out to me that HÜNEFELD detected a trace of it in the waters of Gastein in the Tyrol,† and that PLANIAVA found gr. 0·07 of the fluoride in 10,000 grains of the water of Lukatschowitz in

* Lehrbuch der Chemie, vol. ii., p. 607.

† Bulletin des Sciences Médicales, vol. xvii., 425. From Jahrbuch der Chemie und Physik, xxii. 458.

Moravia.* MIDDLETON has found it in the London pipe-water, and in three other waters, the localities of which he does not mention; but as the experiments were made in London, they were probably English. Traces of it have been found in other waters also.

I was induced to search for it in the water used in certain of the breweries in Edinburgh, in consequence of learning that these rapidly corrode the thermometers employed to regulate the temperature of the boilers and vats. The fact was first mentioned to me by a gentleman, who, before I made any trials on the subject, inferred that the corrosion of the glass must be owing to the presence of a fluoride in the water. I discredited the statement when I first heard it, supposing that an incrustation or deposition of sulphate and carbonate of lime had been mistaken for a true corrosion. I thought it impossible, moreover, that fluoride of calcium, even if it were present, could act upon glass. But in the course of the experiments already detailed, I had once occasion to notice that a new Berlin porcelain basin, in which a considerable quantity of the aqueous solution of fluoride of calcium was boiled down, had its glaze completely removed. On observing this fact, I applied to our intelligent instrument-maker, Mr STEVENSON, through whose hands the greater number of the thermometers used by the Edinburgh brewers pass, in the course of receiving necessary repairs. He informed me that he was quite familiar with the rapid dimming of the thermometers, and that it was a true corrosion; in proof of which he gave me two pieces of broken thermometers, which I shew the Society. They are certainly abraded, and present a surface like that of ground glass. The roughening which is so manifest was not the result of friction against the sides of the brewing vessel, or any other kind of mechanical action; for the corroded part of the thermometer-stem was enclosed in a brass-tube, and completely protected from external violence. It is proper to mention that the workmen in some of our breweries are in the practice of scraping the stems of their thermometers, to remove the deposit of lime-salts which rapidly gathers on them, and are ready to affirm that the apparent corrosion is an abrasion occasioned by their own knives. To guard against the possibility of any deception having occurred in this way, I visited the brewery of Mr CAMPBELL, situated in the Cowgate, behind Minto House, and was shewn by his manager a thermometer which had never been scraped with any instrument, and had been in use only a few weeks, but was nevertheless so dimmed, that it required to be dipped into water in order to confer upon it a temporary transparency, before the included mercury could be distinctly seen. Mr STEVENSON informs me that he finds the protected parts of the thermometer-stems, which are enclosed in brass-tubes, as much corroded as those which are exposed.

* Bulletin des Sciences Medicales, vol. xvii. p. 425. From Zeitschrift für Physik und Mathematik.

It seemed well worth while to seek for fluorine in one of these waters. I obtained accordingly from Mr CAMPBELL'S brewery, a portion of the abundant deposit, consisting chiefly of sulphate and carbonate of lime, which collects with great rapidity in the boilers. It was treated with nitric acid, the dissolved portion poured off, neutralized with ammonia, and precipitated by nitrate of baryta. The precipitate, after being washed and dried, was warmed with Nordhausen sulphuric acid, in a lead basin; a square of waxed plate-glass, with characters traced through the wax, being laid as a cover over it. In this, as in all other experiments of the kind, a wall of wax was raised on the edges of the upper side of the glass, so as to retain a portion of water sufficient to keep the plate cool, and condense the hydrofluoric acid on it. This simple, but useful device, I borrowed from Dr DAUBENY.* Three squares of glass were very distinctly, though not deeply, etched in this way.

Fluorine, then, was present in this water; and the fact has an interesting relation to the circumstance pointed out to me by Mr ROSE, that the well from which it was obtained is sunk through a bed of sandstone, containing much mica, a mineral in which ROSË,† TURNER, GREGORY,‡ and other analysts, have found between 1 and 2 per cent. of fluorine. In reference to the corrosion of the brewery thermometers, however, I wish it to be distinctly understood, that I do not seek to refer the whole abrasion of the glass to the action of a fluoride dissolved in the water in which they are immersed. The well-known experiments of LAVOISIER, made in the end of last century, proved that even distilled water, if long boiled upon glass, can corrode it. Every chemist is familiar with the rapid action of solutions of the fixed alkalis, and of phosphate of soda on flint-glass. The inferior kinds of bottle-glass, especially when containing too little silica and excess of lime, have been shewn by FARADAY‖ and WARRINGTON to suffer corrosion by the action of wine, and of diluted hydrochloric, sulphuric, and tartaric acids; and it would be rash to suppose that these are the only re-agents that can act upon artificial silicates, especially upon those which contain excess of basic oxides.

On the other hand, it is impossible not to connect the fact that the thermometers are corroded, with the circumstance that the water which occasions this corrosion contains fluoride of calcium. The other constituents of the brewery water are chloride of calcium and sodium, sulphate of lime and of soda, carbonate of lime and of magnesia, silica and organic matter; no one of which is known to have any action on glass.

In connection with this fact I may mention, that Mr STEVENSON finds the thermometers used in the breweries in the valley of the Cowgate much more

* Chemical Society's Memoirs, vol. ii., p. 101.

† POGGENDORF'S Annalen, vol. i., p. 80.

‡ BREWSTER'S Journal of Science.

‖ Chemical Society's Memoirs, vol. ii., p. 247.

rapidly corroded than those employed in the similar establishments in the Canongate. As the action of the water may be supposed to be the same at both places, and the attending circumstances similar, it must be some constituent of the Cowgate wells that occasions the difference. It may be the fluoride of calcium.*

To conclude this part of the subject, I may state that Dr CHRISTISON informs me, that he has frequently had occasion to notice that considerable quantities of natural waters evaporated to dryness in glass basins, permanently destroyed the transparency of the latter. From all that has been mentioned, it will appear that fluorine is likely to prove a frequent, though not an abundant, constituent of ordinary water. If the proposal to construct the pipes of our water-works of glass be put into practice, we may have an opportunity, on the large scale, of testing the truth of this idea.

It follows as a corollary, from the truths already detailed, that fluorine must be present in sea-water. The inference that it must be there, had been drawn by Mr MIDDLETON from the fact, that fluoride of calcium occurs in the shells of marine mollusca.† SILLIMAN junior has come to the same conclusion, apparently without a knowledge of MIDDLETON's views, in consequence of invariably finding the same fluoride in calcareous corals.‡ In the teeth of the walrus and of the shark, the only marine animals I have examined, I found fluorine very distinctly, especially in the latter.

I attacked the problem, however, directly, by examining the water of the Frith of Forth. A portion of the mother-liquor or *bittern*, from the pans at Joppa, near Portobello (three miles from Edinburgh), in which sea-water is concentrated so as to yield culinary salt, was precipitated by nitrate of baryta. The precipitate, after being washed and dried, was warmed with oil of vitrol in a lead basin, covered by waxed glass, with designs on it. The latter were etched in two hours, as deeply as they could have been by fluor-spar treated in the same way, the lines being filled up with the white silica, separated from the glass. To the acknowledged constituents of sea-water, fluorine, then, must now be added.

* As the fact of the frequent presence of fluorine in water must hereafter enter as an important element into all speculations as to the cause of the corroding action of water on glass, I place here on record the result of an accurate quantitative trial on the latter subject.

I am indebted to Mr JOHN ADIE for the particulars of the following experiment, which was made with a view to discover what peculiarity in the structure of glass unfits much of it for optical purposes. A cube of glass, two and a half inches square, was inclosed in a fir box, and fixed immovably in it by pieces of wood. Holes were pierced in the sides of the wooden case, so as to permit the free passage of the water, and the whole was placed in an engine-boiler, supplied with the Edinburgh pipe-water, and left there for six months. During that period the boiler was in action twelve hours each day; the water being under a pressure of 35 pounds on the square inch, and at a temperature of about 260° Fahr. The cube weighed, when first immersed, 9157 grains, and, when taken out, had lost 457 grs., or about $\frac{1}{20}$th part of its weight. It is right to mention, that the condensed steam was returned to the boiler, so that fresh saline matter was only furnished in the water added, from time to time, to supply the waste.

† London Phil. Mag., No. 144, p. 14.

‡ On the Chemical Composition of Calcareous Corals, by B. SILLIMAN junior.—American Journal of Science, vol. i. Second Series.

This fact, besides its interest in relation to natural history, will be welcome to chemists, as adding another link to the lengthened chain of analogies between chlorine, bromine, iodine, and fluorine. In teaching the beautiful law that bodies closely allied in chemical characters occur together in nature, I have always felt the force of the argument weakened, by the absence of fluorine from sea-water where the other members of its class are so abundant. Its detection in bittern removes the difficulty, and adds another to the many relations which are common to the well-marked natural family of simple-salt radicals to which it belongs.

4. *Of the presence of Fluorine in Minerals.*

It remains to connect the initial fact of this paper with the occurrence of fluorine in minerals, and in plants, and animals; and, first, of minerals. I exclude, in the meanwhile from notice, fossil bones, which will be best considered in relation to animal remains.

The solvent power of water over fluoride of calcium is likely to throw some light both on the appearance and disappearance of that substance from particular localities, in relation to geological changes. In connection with this branch of our subject, Mr Rose has reminded me of a phenomenon familiar to mineralogists, namely, the frequent occurrence of quartz with deep cubical impressions on its surface, believed to be casts of crystals of fluor-spar, which had been dissolved away after the deposition of the silica. It is possible that the markings may have been occasioned by galena or iron pyrites, the latter of which we know, in contact with air, can change into sulphate of iron and sulphuric acid, and is then quite soluble in water, and might be readily carried away. Cubical iron pyrites, however, in general is quite permanent, and suffers no change even with the freest exposure to air; and galena is an exceedingly insoluble substance. Mineralogists, accordingly, have universally agreed that the square impressions on quartz are the imprints of fluor, and the very frequent association in nature of the latter with silica, seems to justify their view. It has been a problem, however, what agent has removed the fluor; and it seems not impossible that water may have been the body which dissolved it away. In confirmation of this idea, I may refer to a paper by Mr Robert Were Fox,* in which he describes certain pseudomorphous octohedrons of quartz, "more than an inch in diameter," which "were broken from a copper vein in *Killas*, at the depth of about 160 fathoms from the surface." The crystals were hollow, and many of them contained, hermetically enclosed within them, water, or rather an aqueous saline solution, and numerous pieces of fluor. "Of these," Mr Fox says, "all the fragments are corroded, and indicate, by their rounded edges and indented surfaces, the action of a solvent

* Transactions of the Royal Polytechnic Society of Cornwall, quoted in Edin. Phil. Journal, vol. xl., p. 115.

which penetrated most readily between the planes of cleavage." The contents of the different crystals were not alike, and, unfortunately, Mr Fox, doubtless from the belief that fluor-spar is insoluble in water, made no search for that substance in the liquid procured by breaking the hollow octohedrons. In several cases, however, it precipitated nitrate of baryta and oxalate of ammonia, an action referred to the presence of sulphate of lime, but which may, in part at least, have been owing to fluoride of calcium being present in solution. In the matrix of these crystals, alternate layers of quartz and fluor-spar were found in lines like fortification-agate. When we connect these facts with the solubility both of fluoride of calcium and of silica in water, and with the observed presence of fragments of the former in the hollow crystals, it will be acknowledged that water, if not the true, would at least be a sufficient cause of the phenomena described by Mr Fox, and of the still more familiar one of square impressions on quartz, previously referred to. I have tried the experiment of placing powdered fluor-spar on a filter, and percolating distilled water through it, and have found that the latter precipitated oxalate of ammonia. Whilst, however, I think it cannot be denied that water in contact with fluor-spar must round off the edges of its crystals and dissolve it away, we have no data from which to determine what effect salts held in solution by water may have in increasing or diminishing its solvent power.

It is necessary to mention here that fluorine is already known to occur in many minerals besides fluor-spar. It has been found in hornblende, as well as in mica, in apatite, wavellite, wagnerite, urinite, phosphorite, &c., along with phosphate of lime. BERZELIUS and ROSE* found it in sulphate of baryta; so did MIDDLETON. I have found it there also; I have likewise obtained it in one case from gypsum. It has been found in other sulphates, and MIDDLETON has frequently detected it in carbonate of lime. In general, it may be expected to occur along with the insoluble salts of baryta, strontia, and lime, and probably also with those of lead, especially when these are of aqueous origin. MIDDLETON'S detection of fluoride of calcium in the shells of marine mollusca, and SILLIMAN'S recent elaborate analysis of corals, which resulted in shewing that in nine different species (the only ones in which it was sought for) fluoride of calcium occurred, lead directly, as the latter gentleman has indicated, to the conclusion, that shell, coral, and metamorphic limestones, may be expected to contain that salt. When to these sources of fluorine we add animal remains, especially their bones and excretions, but in truth their whole mass, it will be manifest that in all parts of our globe, water may meet with fluorine, and carry it into the tissues of plants and animals. This remark leads directly to its occurrence in the two latter; and, first, of plants.

* Griffin's Rosé's Quantitative Chemistry, p. 348.

5. *Of the presence of Fluorine in Plants.*

Comparatively few examinations of plants have yet been made, in reference to the occurrence of fluorine in them; but these, on the whole, have been satisfactory. SPRENGEL appears to have been the first to suggest the likelihood of its presence in vegetables, but failed in detecting it in any of them, and referred his failure "to its existing in such a state of combination as caused it to be dissipated by the heat necessary for expelling the carbonaceous matter, so that it could not be detected in the ordinary method."*

Dr DAUBENY "ascertained that no sensible action is exerted on glass by heating, with sulphuric acid, the earthy phosphates present, in twelve pounds of barley."† I was equally unsuccessful with the ashes of Kanaster tobacco, and of peas, and with those of charcoal and of coal. I ascribe the failure, however, not to fluorine existing in a peculiar state of combination in plants, but to the presence of silica, which, when in any quantity, makes the detection of fluorine very difficult. I took no measures to separate the silica in the few experiments I made on the subject, and SPRENGEL and DAUBENY appear to have omitted the same essential preliminary. Dr WILL of Giessen, who kept this point carefully in view, states that "careful experiments, conducted under his own superintendence, by Messrs JAMES MÜLLER and BLAKE, severally, have shewn that the ashes of French barley, grown in Switzerland, contain very distinct traces of it; both straw and grain were employed."‡

To WILL the credit of first finding fluorine in plants is entirely due. As barley, however, contains a large amount of calcareous phosphates, which the fluoride of calcium has been supposed to accompany in a peculiar state of combination, I thought it well to examine the ashes of a plant containing little phosphate of lime, and which might be considered as having derived any fluorine it contained directly from the water its roots absorbed. I chose for this purpose the crudest American potashes, which, as they are obtained in part by burning the young and succulent branches of trees, should contain portions of all that is soluble in the sap. A pound weight of ashes was supersaturated with hydrochloric acid, the liquid poured off, neutralised with ammonia, and precipitated by nitrate of baryta. The precipitate washed and dried, when treated with Nordhausen sulphuric acid in a lead basin, in the way already described, etched glass distinctly.

Whether the fluorine found in plants is essential to them, and serves some purpose in their organization, or merely circulates in their sap, as other soluble matters do, without being appropriated by the living organism, cannot be deter-

* Chemical Society's Memoirs, vol. ii., p. 103. † Ibid. ‡ Ibid, p. 182.

mined in the present state of our knowledge. But fluorine may be expected to occur in the fluids of all the higher plants. On this subject WILL observes, "fluorine occurs in the teeth and bones of animals, having been derived by them from vegetable food; it will doubtless, therefore, exist still more abundantly in the ashes of plants."* This expectation was probably founded on the supposed insolubility of fluoride of calcium in water, and is not likely to be fulfilled. Animals which may derive fluorine both from their solid food, whether animal or vegetable, and likewise from the water they drink, are likely to excel plants in the proportion of fluorine they contain.

6. *Of the presence of Fluorine in Animals.*

As there exists, then, a twofold source of fluorine for animals, we may anticipate its occurrence in various parts of their structures. Passing by for the moment, as a disputed point, the occurrence of fluoride of calcium in recent bones, and excluding the consideration of its presence in shells and corals, it may be noticed that the urine of man is the only animal product in which fluorine has been quite certainly ascertained to occur. GAY LUSSAC appears to have been the first who suggested the probability of fluorine being found in the secretion of the kidneys,† but he did not make any experiments on the subject. With the precipitate obtained by adding lime water to human urine, BERZELIUS etched glass distinctly; and from the period of his experiments, fluorine has been ranked among the normal, or at least the occasional, ingredients of the fluid in question. The fact, however, has always been referred to with a kind of hesitation, and had it rested on the authority of any less distinguished chemist than the great Swedish one, it would have dropped, I fear, out of notice altogether.‡

REES repeated BERZELIUS' experiment, but quite unsuccessfully, and, as in the case of bones, affirms that fluorine cannot be found in urine. I made but one trial on the subject, but it was so decisively confirmatory of BERZELIUS' original result, that it seemed unnecessary to repeat it. About 50 ounces of urine (*urina potus*), were precipitated by nitrate of baryta, which, for reasons already fully detailed, is preferable to lime, as forming a less soluble salt with fluorine. The precipitate, consisting chiefly of sulphates and phosphates, was collected on a filter, and dried without washing. Warmed with Nordhausen acid, it corroded glass deeply, the lines being filled with white silica, exactly as if fluor-spar had been used. In none of my experiments, except with the dissolved fluor, and with sea-water, has the etching been so distinct as it was in this case. I recommend those who wish to succeed in this experiment to employ what the older physiologists distinguished as *urina potus*. In districts where fluorine is not abundant in the soil or waters, it may not so frequently

* Chemical Society's Memoirs, vol. ii. p. 182.

† Ann. de Ch., t. lv.

‡ SIMON'S Animal Chemistry, vol. ii.

occur in the bodies of animals, as it does in those of the inhabitants of other localities. The experiment in question was made in Edinburgh.

It could not be doubted, after the facts I have detailed, that fluorine would be found in the two great formative liquids of the animal body, blood and milk; I have found it in both. So far as I am aware, it has hitherto been overlooked in all the analyses that have been made of these liquids; probably it has not been sought for.

I employed the blood of the ox, and in two cases obtained markings on glass, which only become visible when breathed upon, but are then quite manifest. In the third the glass was distinctly, though faintly, corroded.

That others may know exactly how the experiment was made, I may mention, that in the most successful case, about 128 ounces of blood were taken, which had been freed, by stirring, from much of its fibrine, and was boiled till the liquid solidified. The broken coagulum was burned in a large crucible, the ashes boiled with dilute muriatic acid, and the liquid filtered and evaporated to dryness. The residue was then heated to redness to expel chloride of iron, and afterwards washed with a small quantity of water, sufficient only to remove the accompanying chloride of sodium. The insoluble matter, which was in brilliant metallic crystalline scales, was reduced to fine powder, and heated with Nordhausen sulphuric acid, in a basin covered by waxed plate-glass. As the experiment performed in this way gave the fluorine only or chiefly of the serum, we may expect a still more decisive result to be obtained when the whole blood is taken. But it is so difficult to burn the fibrine of blood, that I was content, in a first trial, to experiment chiefly with the serum. As already mentioned, where the entire blood was made use of, glass was marked, but not corroded. Both of the trials, however, with the entire blood, were conducted so as to involve many washings, which were avoided in the third and most successful experiment.

In examining milk, I thought to have saved myself the trouble of boiling down large quantities of that liquid by using cheese, which was burned, and the ashes digested in muriatic acid. The filtered solution was supersaturated with ammonia, and the precipitate which fell (consisting chiefly of phosphates), washed, dried, and tested with Nordhausen acid and glass. In three cases I totally failed to detect fluorine whilst operating in this way. I was not more successful with two quantities of milk treated similarly. I mention these failures to shew the necessity of avoiding methods which imply much washing upon filters, when fluoride of calcium is sought for. The large quantity of liquid made use of in the course of the process referred to, may have held in solution and carried away all the fluoride present; especially as the addition of ammonia could only separate fluoride of calcium previously dissolved by an acid. In the last and successful attempt to detect fluorine in milk, the hydrochloric solution was neutralised with ammonia and precipitated by nitrate of baryta. The precipitate, which was

slightly washed, etched quite distinctly. A still better process would be that followed with the blood, where washing was reduced to a minimum. These experiments throw no light on the condition in which fluorine exists in blood or milk; nor would it be easy to ascertain in what state of combination it occurs. In the meanwhile we may suppose it to be present as fluoride of calcium.

There is one other animal fluid, the last towards which suspicion as to its containing fluorine was likely to have been entertained, in which hydrofluoric acid has been declared to occur, namely, the gastric juice. BRUGNATELLI states that fragments of rock-crystal and agate inclosed in tubes, and introduced into the stomachs of hens and turkeys, were found in ten days to have lost 10 or 12 grains in weight. TREVIRANUS, also, is said to have found that chyme from hens' stomachs corroded porcelain capsules. These experiments are thought to prove the presence of free hydrofluoric acid in the gastric juice. TIEDEMANN and GMELIN placed the gastric juice of ducks in platina crucibles covered with plates of glass, having lines traced on them through a coating of wax; but found no action on the glass after 24 hours digestion with the aid of heat.* That hydrofluoric acid exists in the gastric juice of birds, in such quantity as to round off pebbles with the rapidity implied by BRUGNATELLI's observations, is not probable; but the force which is able to evolve hydrochloric acid at the stomach from the chlorides which enter the system, is probably adequate, in similar circumstances, to convert fluorides, along with the elements of water, into hydrofluoric acid. We may now look for fluorine in all the animal fluids.

7. *Of the presence of Fluorine in Fossil Bones, and its relation to Animal Life.*

The facts detailed in the preceding sections remove much of the difficulty that has attended previous speculations as to the source of the fluorine found in fossil bones. It will now be conceded, that water must be constantly conveying that element into the organs of animals, whilst other portions are also added in their solid food. Whether this fluorine be supposed simply to travel through the organism, dissolved in the circulating fluid as fluoride of calcium, or as some other salt, and to quit the body as it entered it, without serving any purpose therein; or be imagined to fulfil some important end in relation to the functions of life, it must be expected to shew itself as a very frequent, if not constant, ingredient in the bones. We may quite safely infer that a portion, at least, of the fluorine found in fossil osseous remains must have been present in them when they were parts of a living structure; and when we find only two per cent. of fluoride of calcium in many ancient bones, while certain recent ones contain nine per cent., it may well be doubted whether a trace of the salt has been added during fossilization. Nay, it is a question I think worthy of discussion, whether there may not

* These speculations will be found noticed in BERZELIUS' Traité de Chimie, or in SIMON's Animal Chemistry, vol. ii., art. Gastric Juice.

have been a decrease in the proportion of fluoride, water having washed it out, and dissolved it away. We naturally turn for light on this problem to the consideration of the question, What proportion of fluorine is found in the bones of existing animals? Here, however, we are at once met, as has been already stated, by the most contradictory declarations on the part of able chemists; some affirming that fluorine is found in all bones; others, that none can be detected in any.

When it is considered how extremely simple the process of testing for fluorine is, and how little room there is for difference in manipulative dexterity affecting the result, I am constrained to admit, that, in the meanwhile at least, we must refuse to fluorine the character of being a constant ingredient of bones. On the other hand, it is certainly a very common one, more frequently present than absent; and we may encourage the expectation, that future researches will explain satisfactorily the cause of failure where negative results have been obtained, and prove fluorine to be an ever-present constituent, not only of bones, but of other animal tissues.*

The suggestion, accordingly, of LIEBIG, that fossil bones contain only the fluorine which was added to them whilst parts of living structures, may be found

* Certain of the recent observers have endeavoured to reconcile the conflicting statements of their predecessors in reference to this subject, but with little success. Dr DAUBENY conceives that the failures may have arisen, in part, from the bones examined not having been deprived of their gelatine before being tested for fluorine, so that the animal matter prevented the hydrofluoric acid from acting on the glass.* But on the one hand, FOURCROY and VAUQUELIN, who were unsuccessful searchers for the element in question, pointed out long ago the necessity of burning away the gelatine as a preliminary step, and always did so before looking for fluorine.† On the other hand, Mr MIDDLETON, who found that substance abundantly in bones, simply broke the latter into small fragments, and heated them with concentrated sulphuric acid. He states, moreover, that the time occupied by each experiment was only between five and ten minutes.‡ Dr DAUBENY refers likewise to the presence of salts of volatile acids and salt radicals, such as chlorides and carbonates, from which hydrochloric and carbonic acids are evolved when sulphuric acid is poured on the bones, and which sweep away the hydrofluoric acid before it has time to corrode the glass. He has accordingly described a method of procedure which gets rid of the volatile bodies in question, but only at the risk of losing, in the liberal washings prescribed, much of the fluoride of calcium. It cannot be doubted that the acids which accompany the hydrofluoric, when sulphuric acid is poured upon burned bones, dilute and carry away the body sought for. But FOURCROY and VAUQUELIN's experiments, which were made by distilling bones with sulphuric acid in glass vessels, could have been but little affected by this source of fallacy; and BERZELIUS' successful results were obtained in the very same way.

After trying DAUBENY's process several times, I am constrained to acknowledge that I did not find it give any better results than the simpler one previously in use. If it be deemed requisite to get rid of the carbonic acid of bones before testing for fluorine, I believe it could be done most efficaciously by digesting them, after being burned and reduced to powder, in a solution of tartaric acid, and, after the whole carbonic acid had been expelled, drying up the mass.

REES, taking the opposite view from DAUBENY, has endeavoured to prove that, where fluorine has been supposed to be present, it was in reality phosphoric acid that corroded the glass.§ He acknowledges, however, that this explanation applies only to those cases where the bones were distilled with sulphuric acid, and the product of distillation evaporated to dryness in glass vessels. He regards as unexceptionable, experiments made with platina crucibles covered with waxed glass; and as MIDDLETON's, DAUBENY's, and my own trials were made in this way, and distinct corrosion or etching obtained, his suggestion must be considered as leaving the subject where it found it.

* Chemical Society's Memoirs, vol. ii., p. 100.
† Annales de Chimie, tome lvii., p. 38.
‡ Memoirs of Chemical Society, vol. ii., p. 135.
§ Edin. Phil. Journal, vol. xxviii., p. 93.

quite sufficient in many cases, especially if we add to the food, the drink of animals as a source of the ingredient in question. But when we find fossilized bones containing ten or fifteen per cent. of fluoride of calcium, whilst, at the same time, they have lost to a great extent their original structure, and have acquired a crystalline or mineralized one, it seems highly probable that Mr MIDDLETON's belief that water may have infiltrated that salt into them, will prove worthy of adoption. I cannot, however, agree with him in thinking, that it is enough to shew that water may bring fluoride of calcium to bones, to account for its accumulation in them. Water, as my own experiments prove, may carry away fluoride of calcium from osseous remains, as well as transport it to them. We require to account for its detention in bones, as well as for its conveyance to them. From an experiment made in the laboratory, as well as from their association in nature, I am inclined to think that there is a double phosphate of lime and fluoride of calcium, much less soluble than the latter salt is; and that the production of this compound fixes the fluoride, and prevents its abstraction by water. Further researches will decide this point. Till quantitative analyses of a considerable number both of recent and fossil bones are made, as to the proportion of fluoride of calcium in them, it will be impossible to decide how far individual fossils which contain that salt are to be looked upon as coming under LIEBIG's or MIDDLETON's explanation, or as requiring, as many probably will do, a reference to both. Some, as already stated, may appear to have lost, instead of gaining, fluoride of calcium, during their entombment.

Allusion was made, in the commencement of the paper, to the possibility of a conversion of fluoride of calcium into phosphate of lime having occurred. Few would more gladly see the idea of elemental transmutation realized by natural phenomenon or laboratory experiment than I should do. I can find nothing, however, to support it in the phenomena I have been discussing.

In conclusion, I would observe, that physiologists will doubtless now be tempted to speculate on the possibility of fluorine performing some essential function in living animals. Its occasional absence from their bones would not disprove that it may be necessary for the perfection of certain organs, though not for all. Quantitative analyses appear already to have indicated that the enamel of teeth contains more fluorine than any other part of the body. If that result shall be confirmed, we may suppose that, if fluorine be furnished when the development of the teeth is proceeding, it may be wanting at other periods, without injury to the animal; just as chloride of sodium must be considered as essential to the healthy life of most creatures, though they may be deprived of it for long intervals, without death ensuing.

The small quantity of fluorine found in living structures can be counted no argument against its occasional or constant importance. Quantity, is at best, but a rude measure of the value of an ingredient, in relation to the necessities

of an organism. The law of final causes, in truth, would indicate that *only* a minute proportion of fluorine should occur in any organ; for it would be perilous to an animal to introduce into its system a large quantity of fluorides, which can so readily be changed into the deadly hydrofluoric acid. Such speculations, however, are premature. It will be time enough, when many qualitative, but especially quantitative, researches have been prosecuted, as to the presence of fluorine in animal structures, to consider of what service it is to them; if it be of any.

EDINBURGH, *April* 4. 1846.

XVII.—*Observations on the Principle of Vital Affinity, as illustrated by recent discoveries in Organic Chemistry.* By William Pulteney Alison, M.D., F.R.S.E., Professor of the Practice of Medicine in the University of Edinburgh.

(Read, 2d February 1846.)

Part I.

The most important steps in a science are those which lead most directly to the establishment of principles or laws peculiar to that science itself, and which constitute its claim to be regarded as a distinct branch of human knowledge. It has been long acknowledged that such is the character of many of those phenomena of living bodies which depend on mechanical movements, or changes of position in their particles, and therefore that the laws of vital contractions are to be regarded as equally elementary and distinctive principles in physiology, as the laws of motion or of gravitation in natural philosophy. But a difficulty has been long felt, as to whether a similar claim to peculiarity of the principle on which they depend, can be urged for the chemical phenomena of living bodies.

In laying down the first principles of Physiology and of Pathology, I have, however, uniformly maintained the existence of a power peculiar to living bodies, and to which the term Vital Affinity, as recommended by several authors, may be properly applied;—a power by which "the elements of nutritious matter are thrown into the combinations necessary for forming the organic compounds, and restrained from entering into other combinations, to which they are prone as soon as life is extinct;—a power which supersedes and counteracts ordinary chemical affinities in living bodies, as completely as vital contractions counteract gravitation or the inertia of matter."—(*Outlines of Human Physiology*, p. 22.) And in delivering lectures on physiology, I always expressed my belief that a time would come, when discoveries in the chemical department of the science,—connecting the ingesta of living bodies with the nourishment of their different textures, and with the nature of the different excretions,—would elucidate the chemical changes which are continually going on in them, and are essential to their living state, as completely as the discovery of the circulation of the blood illustrated many of the conditions of the existence of living animals. It appears to me that this anticipation has been more nearly realized by recent chemical observations, than professed physiologists have yet admitted;—that not only the existence of the principle of vital affinity has been established, but its limits and mode of

action, the cases in which it acts, and those in which it is unconcerned, are to a certain degree defined;—and that a short and general illustration of these points may be of some advantage, if not to the progress of the science, at least to the due appreciation, and proper generalization and expression of the knowledge which has been already acquired.

To shew the importance of this inquiry, I need do no more than quote a single sentence from CUVIER, with a statement which is nearly a commentary upon it by Professor WHEWELL. "It belongs to modern times to form a just classification of the vital phenomena; and upon the zeal and activity given to the task of analysing the *forces* which belong to each organic element, depends, according to my judgment, the advancement of physiology."* "As the vital functions became better understood, it was seen more and more clearly at what precise points of the process it was necessary to assume a peculiar vital energy, and what sort of properties this energy must be conceived to possess. It was perceived when, and in what manner and degree, mechanical and chemical agencies were modified, overruled, or counteracted by agencies *which must be hyper-mechanical and hyper-chemical*." "In attempts to obtain clear and scientific ideas of the vital forces, we have first to seek to understand the cause of change and motion in each function, so as to see at what points of the process *peculiar causes* come into play; and next, to endeavour to obtain some insight into *the peculiar character and attributes of these causes*."†

When we say that the chemical changes which take place in living bodies are elucidated, we mean, of course, that they are referred to general laws, by which the phenomena observed in this department of Nature are found, by experience, to be regulated. And when we say that these are laws of vitality or of vital action, we mean merely, that they are laws deduced from the observation of phenomena peculiar to the state of life,—taking for granted that it is always possible to describe, and practically to distinguish, those substances which we call living, from inorganic or dead matter; and that the only correct definition of vital principles or vital powers, is, that they are the laws or the powers which regulate the phenomena that are peculiar to the state of life. They are the general expression of the results of the observation, and generalization of the facts, which are observed in this department of nature, and which are ascertained to belong to this department alone.

We are not, indeed, justified in asserting the existence of laws peculiar to the state of life, merely by the *negative* observation, that the phenomena referred to them are *inexplicable* by any known laws of inorganic or dead matter; we must have the *positive* observation that they are *inconsistent with*—that they take place

* Hist. des Sciences Naturelles depuis 1789, p. 218.
† Philosophy of the Inductive Sciences, vol. ii., pp. 39 and 47.

in despite of—the laws which regulate the changes of dead matter. It is thus that we are led to ascribe the visible movements of living bodies to vital powers; not because we do not perceive how gravitation, elasticity, or any other known causes of movement in dead matter should produce them, but because we do perceive, that, in the circumstances in which we see these motions, all those principles, deduced from the observation of dead matter, would determine either rest, or motion in a different direction from that which really takes place.

I formerly laid before this Society the grounds of an opinion, then much disputed, but now, I think, pretty generally admitted, that there are Attractions and Repulsions, as well as contractions, peculiar to the living state: chiefly, but not exclusively, observed at those parts where chemical changes are effected in living bodies, and connected with these changes; and, without reference to this general fact, I maintain that it is impossible to have a right understanding of many phenomena of essential importance in physiology and pathology.*

But the general principle is obviously equally applicable to chemical changes as to mechanical movements. It is not, indeed, so easy to ascertain, in regard to chemical changes in living bodies, that they are truly inconsistent with the chemistry of dead matter; the science must be allowed to make some progress before this can be confidently asserted in regard to any individual chemical change; but no one can doubt that, as science advances, it must become possible to say with certainty, whether the chemical changes in living bodies are consistent with those laws which regulate chemical changes elsewhere, or not; *i. e.*, whether the same chemical elements can be so brought together by the chemist, as to tend to the same combinations as are found in living bodies; or whether, in his hands, they will enter uniformly into other combinations, and form different compounds.

Farther, it appears to me that, even before any of the recent discoveries, it might be legitimately inferred from facts already known, that this last description is truly applicable, in some cases, to the chemistry of living bodies. It was known, for example, that when water, impregnated with carbonic acid and with a small proportion of ammonia, is brought into contact with vegetable substances, in a certain stage of their existence, the elements of these bodies rapidly combine so as to form starch, albumen, and oil, which are added to the substance of the vegetables,—that under no other circumstances can water, carbonic acid, and ammonia, or their elements, be made to form these compouds,—and farther, that after a time, when brought into contact, at the same temperature, with the same vegetable substance in an ulterior stage of its existence, they will form no such com-

* Professor WHEWELL, in his instructive abstract of the general principles ascertained in Physiology, regards it as established, chiefly on the authority of MÜLLER, in regard to the vital force concerned in assimilation and secretion, that " it has mechanical efficacy, producing motions, &c. But it exerts at the same point both an attraction and a repulsion, attracting matter on one side and repelling it on the other; and in this it differs entirely from mechanical forces."—*Philosophy of Inductive Sciences*, vol. ii., p. 51. See also CARPENTER's *Manual of Physiology*, § 597, *et seq.*

pounds, but will aid and participate in the successive changes to which vegetable matter is liable after the phenomena of its living state are over, and of which the ultimate result is, the resolution of that matter into its original constituents. And from these facts it seems quite reasonable to infer, that during the former, or what we call the living state of the vegetable, certain affinities peculiar to the living state—*i. e.*, certain vital affinities—actuate the elements of which it is composed.

In asserting the existence of vital affinities, we do not, in the first instance, give any opinion whether it is by the addition of certain chemical attractions, or by the suspension of others, during the living state, that the chemical changes peculiar to that state are effected; we assert nothing more than what is, as I think, correctly stated in the following sentence of LIEBIG:—"The chemical forces in living bodies are subject to the invisible cause by which the forms of organs are produced." "The chemical forces are subordinate to this cause of life, just as they are to electricity, heat, mechanical motion, and friction. By the influence of the latter forces, they suffer changes in their direction, an increase or diminution of their intensity, or *a complete cessation or reversal of their action.*

"Such an influence, and no other, is exercised by the vital principle over the chemical forces."

"The equilibrium in the chemical attractions of the constituents of the food is disturbed by the vital principle, as we know it may be by many other causes. The union of its elements, so as to produce new combinations and forms, indicates the presence of *a peculiar mode of attraction*, and the existence of a power distinct from all other powers of nature, viz., the vital principle."—(*Organic Chemistry, &c.*, pp. 355, 357).

In these passages I think that LIEBIG has expressed himself with perfect accuracy; but in other parts of his writings he uses language in regard to the nature and results of chemical changes in living bodies, which seems to me vague and speculative, and even inconsistent with what he had stated in the passages just quoted, *e. g.*, when he says that "the ultimate *causes* of the different conditions of the vital force in nutrition, reproduction, muscular motion, &c., are *chemical forces.*"—(*Organic Chemistry*, p. 10).

The following sentence by MÜLDER expresses the very same idea, although it might be thought, from the manner in which this author expresses himself against any introduction of the vital principle in this department of physiology, that he considers all the chemical changes in living structures to be referable to the same laws as in inorganic matter.

"By a small organ of a plant *a force is exercised*, exciting forces which slumbered in the carbon, oxygen, and hydrogen, or rather *modifying the forces which existed in these*, so that 12 equivalents of carbon unite with 10 of hydrogen and 10 of oxygen; and from 12 equivalents of carbonic acid ($12\,CO_2$) and 10 of water

(10 H O) starch is produced, 12 C 10 H 10 O, 24 of oxygen passing off."—(*Chemistry of Vegetable and Animal Physiology*, p. 67).*

But it is important to fix our attention, for a short time, on the instances adduced by MÜLDER, of the formation of starch, or some of its allied compounds, out of carbonic acid and water, by the combination of the carbon of the acid with the elements of water, and the expulsion of the oxygen of the acid; because this is the grand and fundamental power, which must have been called into operation when organized structures were first created on earth, and on the continued exercise of which the existence of all such structures, vegetable and animal, is still essentially dependent; and because the simplicity of the process makes it a fit case for considering the question, whether the power here named is strictly entitled to the epithet vital; or whether, as some eminent physiologists in this country maintain, the idea expressed by that term is incorrect and unscientific.

The opinion of those who oppose the doctrine of vital affinity, is thus distinctly stated in the Anatomy of Drs QUAIN and SHARPEY:

"Although the products of chemical changes in living bodies for the most part differ from those appearing in the inorganic world, the difference is nevertheless to be ascribed, not to a peculiar or exclusively vital affinity different from ordinary chemical affinity, but to common chemical affinity, operating in circumstances or conditions which present themselves in living bodies only; and undoubtedly the progress of chemistry is daily adding to the probability of this view."

I consider this to be a hasty and ill advised statement; and to shew this, I request attention, *first*, to the perfect simplicity of the apparatus by which this change is effected. "In all plants," says MÜLDER, "there exists a small organ, of the most simple form, although employed by nature for the most varied purposes. It is a small filmy sac, a thin membrane, which encloses a small space, which it enables to communicate with the exterior space through invisible pores. These little sacs or cells are the chief organs of plants. A countless multitude of them, grouped together, forms the whole bulk of the plant, so that if every thing except the cells be destroyed, the shape and size of the plant are not in the least changed or diminished."

Into this simple apparatus in certain parts of plants, water, impregnated with carbonic acid, is introduced, while the plants exhibit the phenomena of life; and

* In the foregoing and other translations from recent German writers, the word *force* is used in a sense which I think would be much better expressed by the term *power* or *property*, merely on this account, that the English word force, in physical discussions, has usually a precise and limited meaning assigned to it, as a cause capable of producing visible motion, and of which we have a measure, either in the velocity or in the quantity of motion which it can excite; whereas the term power or property, applied to any material substance, has a more general meaning, as simply the cause of change of any kind, and is therefore applicable where the result of the property ascribed to any substance may be very different from visible motion.

let us next observe the intensity of the action by which the carbonic acid is there decomposed, the carbon attached to the elements of the water, and the oxygen set free. "This is done by a power," says LIEBIG, "to which the strongest chemical action cannot be compared. The best idea of it may be formed by considering, that it surpasses in power the strongest galvanic battery, by which we are not able to separate the oxygen from carbonic acid. The affinity of chlorine for hydrogen, and its power to decompose water, under the influence of light, and set its oxygen at liberty, cannot be considered as nearly equalling the power and energy with which a leaf, separated from a plant, decomposes the carbonic acid which it absorbs."—*Organic Chemistry*, p. 134.

Next let us observe the extent to which this energetic power is exercised by living plants. Perhaps the most accurate idea of it may be formed from attending to the statement of THEODORE DE SAUSSURE, that on a mean of 54 observations made in a country district, the proportion of carbonic acid in the atmosphere during the night was to its proportion in the day-time as 432 to 398, *i. e.*, the carbonic acid existing in the atmosphere was found to be diminished very nearly 10 per cent. in a few hours of every day; and for this diminution we know no cause, except that this power of the green parts of vegetables, of decomposing the carbonic acid of the atmosphere, is exercised only under the influence of light.*

Now if a power of this extraordinary energy and extensive operation, and acting in so very simple a manner, were really to be regarded as depending only on ordinary chemical affinities, exerted under peculiar conditions, it might surely be expected, that the chemist might so regulate the conditions under which he might bring together carbonic acid, air, and water, as to exhibit some traces of this power, and effect some decomposition of the carbonic acid and evolution of oxygen. But we know, not only that this cannot be done, but that when air, water, and carbonic acid, are introduced into the very same vegetable cells, within half an hour after they have exhibited this phenomenon, at the same spot, under the same light, and at the same temperature, they will not only fail to exhibit the same change, but will uniformly exhibit the very reverse, *i. e.*, the absorption of oxygen and the formation and evolution of carbonic acid.

Nay, we know that it is only in certain cells of the living vegetable, that this peculiar chemical change, under the action of light, is effected; the same fluid, introduced into cells composed of the same material in the parts of fructification, undergoes no such change; but, on the contrary, gives occasion only to the reverse process, the absorption of oxygen and evolution of carbonic acid.†

Then it is to be remembered, that this complete inversion of ordinary chemical affinities, in the case of the living plant, is only one of several cases to

* See MACAIRE'S Memoir of THEODORE DE SAUSSURE, in Edinburgh Philosophical Journal, vol. xl. p. 31. (Jan. 1846,)

† THEODORE DE SAUSSURE, in Edinburgh Philosophical Journal, vol. xl. pp. 22, 23.

be afterwards noticed, where we see chemical compounds uniformly formed in living bodies, quite distinct from any that can be formed by the chemist from the same elements, and quite distinct from those to which the same elements uniformly revert, after the phenomena of life are over.

Lastly, we must remember, when we see this apparent inversion or alteration of the ordinary chemical relations of matter, taking place in the interior of living bodies, that in that scene, by the admission of all, matter comes under the dominion of *mechanical* laws, which operate in no other department of nature; so that it is quite conformable to analogy to suppose that its chemical relations will undergo a similar modification.

When all these considerations are duly weighed, I cannot perceive what further evidence can be required in order to justify the expression which I have quoted from Liebig, viz., that the "*new combinations*," as well as the forms, assumed by that matter which goes to the composition of organized beings, "indicate the existence of a power distinct from all other powers of nature, viz., the vital principle;" *i. e.*, that the vital principle regulates the changes of chemical composition, as well as the changes of position which the particles of that matter undergo; which is more simply expressed by saying, that there are vital affinities as well as vital contractions and attractions.

But even if we are to regard it as doubtful whether or not ordinary chemical affinities can determine, under any conditions, this decomposition of carbonic acid and evolution of oxygen by its contact with carbon and the elements of water, I maintain that it is sound philosophy, when we see this and other rapid and extensive and important chemical changes, essentially different from those which the same elements present under other circumstances, uniformly attending the phenomena of life in vegetables,—to investigate and generalize the laws by which these changes are regulated, as laws of living action, leaving it open to future inquirers, if they can, to resolve them into other laws of more general application. For although I acknowledge the force of the aphorism, "Frustra fit per plura quod potest fieri per pauciora," still I apprehend, that in every case to which this aphorism is applied, the *potest fieri* must be established, not by conjecture, but by experiment; otherwise we fall into the error, so strongly condemned by Bacon and others, of prematurely generalizing, and supposing the laws of nature to be fewer and more comprehensive than they really are.

Having thus, in reference to this first and simplest example, vindicated the soundness of the principle which I propose to illustrate, I think we may next shew, that the main object of inquiry in the chemical department of physiology is more simple and precise, and the extent of that inquiry, necessary to elucidate most questions in physiology, much less than might be supposed from the multiplicity of details, of which what is called the science of organic chemistry is made

up. After what LIEBIG calls the "peculiar mode of attraction" which operates in living bodies, has led to the formation of certain organic compounds, these compounds lose their connection with living bodies, become liable to an infinite number of changes and decompositions, and thus give origin to an infinite variety of substances—generally of temporary duration only, because retained in their form by attractions of no great intensity—applicable to many useful purposes, *but foreign to the inquiries of the physiologist.* He is concerned only with the chemical changes which take place *in living bodies themselves, and during the state of life;* and the results of recent inquiries seem to me sufficient to shew, that the fundamental and peculiar arrangements of chemical elements there observed are less numerous, and the laws regulating them more simple, than they have usually been thought.

In considering this subject, we are enabled, by the results of the inquiries of geologists and physiologists, to revert to the period of the introduction of living bodies into the world, and reflect on the conditions then assigned for their existence. We are justified, by reason, in allowing the imagination to fall back on the time when this Earth rolled through space an inanimate mass; and if any minds, besides that of the Great Ruler of the universe, were connected with it, they did not hold their connection through the medium of any organized structure. For I believe we are justified in laying down these propositions as established, *first,* That the simply physical arrangements of this globe were completed before any organised beings were created; *secondly,* That vegetables were created and lived chiefly on the atmosphere, fixing large quantities of carbon from it on the earth's surface, before animals were called into existence; and, *thirdly,* That at whatever time their existence began, either the first living being of every species, vegetable and animal, or the first ovum from which that being was developed, must have been formed in a manner wholly different from that in which any living bodies, at least of the higher orders, are now reproduced; *i. e.,* that they must have been formed in a manner strictly miraculous, and, of course, beyond the limits of physical science.

But although we cannot ascend higher, in prosecuting this subject, than to inquire in what manner the first plants, or the germs of the first plants, were enabled so to act on the inorganic matter around them as to extract from it the materials, first of their own growth and sustentation, and afterwards of all other organized beings,—yet in the inquiry, thus limited, important progress has been made. From the time when these nascent organized bodies sprung into existence, we must regard it as an ultimate fact, that they were endowed with the power, which all the vegetables that have succeeded them have exercised, of so modifying the attractions existing among the particles of matter, as to cause many of these particles from the air and the water immediately surrounding them, to enter into their substance, by their roots and leaves, or by the organs which soon

became their roots and leaves, and then to arrange themselves there, in those peculiar forms by which the numberless species of the vegetable world are characterized. I apprehend we must also regard it as an ultimate fact, that they were endowed with the power of so modifying the chemical relations of the elements composing those absorbed matters, as to select and retain certain of these elements, and allow others to pass away from them, to decompose the carbonic acid, fix the carbon, and invest it with those peculiar affinities for the water, the hydrogen of the water, and a few other elements, contained in the surrounding media, by which all the proximate principles, first of vegetables and then of animals, and therefore the whole substance of organized beings, are formed.

But it is important to have a precise exposition, although not an explanation, of the power thus exercised by the first plants; and it is still more important and satisfactory to be able to shew how, by the exercise of these and analogous vital powers, the atmosphere must have been gradually changed, the proportion of carbonic acid in it diminished, and the proportion of oxygen increased; how it became fitted, and is kept fitted, for the residence, first of cold-blooded and then of warm-blooded animals; how most of the other conditions of existence of these animals have been, and still are, continually prepared for them by these living actions of vegetables; how all the variety of the textures of all organized bodies, from the origin of vegetables to the death and decomposition of animals, are continually formed and maintained; and how, in both divisions of organized beings, Nature has provided, not for the permanent existence, but for the development and decay of successive generations of individuals, and thus for the perpetuation of the species. These are the subjects of investigation in the chemical department of physiology; and if it can be shewn, that, by a few simple laws, regulating what we call vital attractions and affinities, *i. e.*, modifying, in organized bodies, the attractions and affinities to which matter is everywhere liable, provision is made for all this succession and continual renewal of the phenomena of life; then, although we cannot explain the introduction of living beings into the world, any more than we can explain the dissemination of the stars throughout space,—although we must always regard the appearance of organized bodies on the earth's surface as the clearest indication which human knowledge presents of the subjection of the universe, not only to general laws, but to an arbitrary Will, superior to these laws and changing them at pleasure,—yet I think it may be said that we have nearly as clear an insight into the designs and arrangements of Providence for the maintenance of living beings upon earth, and for the eternal reproduction of them there, so long as these laws shall be in force, as we have into those by which the movements of the heavenly bodies are directed and controlled.

I. Our first business is to study the facts that have been ascertained in regard to the simplest form of chemical change to which the term vital may be applied,

which is merely the *selection*, by a portion of a living structure, of some one substance existing in a fluid, and the consequent attraction of this to a particular part of the structure, while other materials, equally presented to that living part, are excluded.

We need not here enter into the question, on which chemists and agriculturists are not yet agreed, whether the nourishment of plants, in the present condition of the earth's surface, does or does not require the pre-existence, in the soil, of organic compounds, resulting from previous living beings, which are absorbed from it. But we may justly give the name of vital attraction or affinity to that power by which certain saline matters, dissolved in the compound fluid which is absorbed, are retained in the substance of the plant, while others are returned to the soil. "The experiments of MACAIRE PRINCEP," says LEIBIG, "have shewn that plants, made to vegetate with their roots, first in a solution of acetate of lead, and then in rain-water, give back to the latter all the salt of lead which they had previously absorbed. Again, when a plant, freely exposed to the air, rain, and light, is sprinkled with a solution of nitrate of strontian, the salt is absorbed, but is again separated by the roots, and removed farther from them by every shower of rain, so that at last not a trace of it is to be found in the plant. A fir-tree, the ashes of which were analysed by a most accurate chemist, grew in Norway, on a soil to which common salt was conveyed in great quantity by rain-water. How did it happen that its ashes contained no appreciable quantity of salt, although we are certain that its roots must have absorbed it after every shower? We can explain this only by the observations above referred to, which have shewn that plants return to the soil all substances unnecessary to their own existence; and we are thus led to the conclusion that the alkaline bases, existing in the ashes of plants, must be necessary to their growth, since, if this were not the case, they would not be retained." (Ib. p. 103, 4.) Another inference is at least equally obvious, that plants have the power of fixing and retaining within them, those matters which are suited or essential to their composition; and this power we regard as the simplest form of vital affinity. It may be said, that the alkaline bases are thus fixed in plants, because they enter into combination with organic acids, and that, therefore, it is the formation of these acids, not the retention of the bases which combine with them, that is truly the vital change. But this does not apply to other saline matters contained in vegetables, which must have been taken up from the soil in the same state in which they are found in the plants, *e. g.*, the phosphate of magnesia, which is "an invariable ingredient in the seeds of grasses;" or the silica which is found in certain parts of various plants.

Were it not for this selecting and appropriating power, indicating a simple attraction of some parts of the vegetable for certain earthy or saline matters only, we should find some salts of alumina, as well as of lime or magnesia, in the ashes

of almost all vegetables,—that earth existing in large quantity in all fertile soils, whereas it is "very rarely found in the ashes of plants."

In the animal kindom the same power of simple selection and extraction is more fully exemplified, perhaps most strikingly in the development of many of the lower classes, of which the organization is simple, and the matters deposited from the nourishing fluid remarkably diversified, as in many of the radiata and mollusca, which have horny and earthy integuments. And in all animals, so far as any chemical change is effected in the vital actions of absorption, secretion, and even nutrition, it would appear to be chiefly of this simple kind, consisting in the selection and appropriation of compounds already existing in the fluids on which these functions are performed, not in the formation of new compounds. The chyme which is found in the intestines of an animal during digestion contains all the compounds (albuminous, fatty, and extractive matters) which are found in the chyle absorbed from it, although these are in a different state of aggregation, and associated also with other matters which are not absorbed. Since it has been ascertained that the compounds which used to be thought peculiar to the greatest secretions in the body, the bile and the urine, pre-exist in the blood, and are only evolved at the liver and kidneys,—accumulating, therefore, in the blood, when the secretive action of these organs is suspended,—it has become obvious that the main office of these organs is not *formative*, but only *attractive*, to extract from the blood compounds already existing there. And, although there is one material extensively employed in the formation of animal textures, viz., gelatin, which cannot be detected in the blood; yet, as this is the only material so employed which cannot be found there, and as a substance very closely resembling it is found there under certain circumstances, we may assert that in animals by far the greater part of the act of nutrition, numerous and diversified as the compounds forming the solid materials of animal bodies may be, is likewise of this simple kind.

We may consider, then, the selection and extraction, from a previously existing compound fluid, by the agency of a previously existing compound solid, of certain portions of that fluid already elaborated, as a chemical action, essential to all living beings, and so peculiar to them that it may be, at least with high probability, termed an exercise of a vital affinity. And, in regard to this simplest kind of such action, the following points may be considered as ascertained:—

1. It seems to be always performed, in the perfect vegetable or animal, by an agency, not of vessels, as was formerly supposed, capable of a vital contraction, and of changing the nature of their contents by the degrees of that contraction, but of *cells*, either pre-existing in the solid structure, or carried about in the nourishing fluid, and having the name of the globules or corpuscules of that fluid. Most of the textures seem to be formed by the gradual transformation, elongation, or flattening of cells, which have sprung from nuclei at-

tached to previously existing cells; and it seems to be only by the successive formation, distension, rupture, and disappearance of cells, that secretions make their way into the excreting ducts of glands, or on the surface of membranes.

The dependence of all living structures, and of all secretions, not simply on vascular action, by which nourishing fluids are circulated through them, but on *cellular action*, by which this nourishing fluid is changed, appropriated, and retained, or restored to the circulation, is the great step which has been recently gained in physiology by the use of the microscope; and seems to me to be one of the clearest proofs of the dependence of all vital phenomena, on peculiar attractions and repulsions, actuating both solids and fluids, and causing motions in the latter,—not on any vital powers residing exclusively in solids. When it is stated, *e. g.* by Mr PAGET, that "the purpose to which the capillaries are habitually subservient, is only the passive one of conveying blood close to those parts of the body which either grow or secrete, and that it is proved that if a part be only able to imbibe the fluid portion of the blood from an adjacent vessel, it nourishes itself as completely, and after the same method, as one whose substance is traversed by numerous capillaries,"*—it becomes obvious that the movements of the fluid portion of the blood, whereby they are applied to growth and secretion, must be determined by causes quite distinct from the contractions of vessels.

2. Living and growing cells, therefore, whether acting on the nourishing fluid just taken into the system (as in the case of the intestinal villi, or the tufts of the placenta), or on the blood brought to them by the capillaries (as in the nutrition of the different textures), appear always to have two functions to perform,—to extract from the nourishing fluid the matter of which they are themselves composed, and to extract from it, likewise, the matter which is contained within them,—*i. e.*, in the organs of secretion, the secreted fluids, and in the different solid textures, that additional matter which is always found, whether lignin, oil or fat, fibrinous, cartilaginous, or bony substance, in a granular or less definite form, incrusting the walls of the cells. It does not appear possible to explain what is distinctly seen in all these cases, without supposing that the pre-existing cells exert a peculiar attraction or affinity, both for the matter by which they are themselves to be nourished, and their successors to be reproduced,—and likewise for another matter, different in the different parts of the structure, by which they are to be filled or distended. And in the case of vegetables, there seems to be this general distinction between the two,—that the former is a matter destitute of azote, and the latter one containing that element.

3. The cell, growing always by attracting to itself a compound matter, existing in the fluid state, and giving it a simple increase of aggregation, the nature of the change which takes place as this matter becomes solid, is simply

* Report in FORBES's Medical Review, July 1843.

consolidation, not *precipitation*, just as the fibrin of the blood, differing from the albumen only in its stronger (vital) tendency to aggregation, is consolidated in its compound form from the liquor sanguinis in the act of coagulation. And thus it happens that these organic solids possess (as was particularly noticed by Dr PROUT) that peculiarity which, in the inorganic world, is observed only in fluids, that even the minutest portion of them contains the very same ingredients (whether earthy or saline, animal or vegetable matters) as is found in the whole mass.

The absence of all crystalline arrangement, and the complex nature even of the smallest particle of an organized body, are the characteristics of matter which has assumed the solid from the fluid form,—not by a chemical precipitation, or separation from matter formerly united to it, but by a vital attraction, subjecting it to "the invisible cause by which the forms of organs are produced."

4. In the next place, we may inquire what difference exists among the cells in different parts of the same structure, to explain the great difference of the compounds which are deposited in them from the same nourishing fluid; and I apprehend, that, on this point, we must come to the same conclusion which CUVIER drew from examining, throughout the animal kingdom, the structure of the different glands, the vessels entering them, and the ducts passing out of them, viz., that *there is no difference* of structure or of composition, corresponding, in the slightest degree, to the great difference of the products which appear. All cells, in the vegetable kingdom, appear to consist of the same matter, cellulose, and in the animal kingdom of the same matter, protein; and in the first instance they are quite similar to one another. When we attend to the early stages of the existence of a living body, when the difference of textures is only beginning to appear, we find only that a fluid passing through similar capillary vessels, and effused into similar cells, in different *parts of the structure*, acquires different properties. And when we carry our inquiries farther back, and observe the first development of cells themselves out of the granular matter inclosed within the sac of the yolk, it appears obvious that the particles of this matter are attracted, not into cells already existing, but *to points where cells are about to be formed*. The facts known as to the evolution of the chick in ovo from the matter that lies in contact with the germinal membrane, sufficiently indicate that the powers which effect the separation of the different component parts of that matter, so as to form the beginning of the different textures and organs, reside, not in pre-existing cells of different composition or structure, but simply in different points of a pre-existing membrane, which, in the first instance, is homogeneous. The expression of LIEBIG, that "the chemical forces in living bodies are subject to the invisible cause by which the forms of organs are produced," when the action of that cause is duly considered, implies, that they are subject to a cause which undoubtedly acts differently at different points of the same matter; but the differ-

ence of the action of which, at these points, is determined by no other condition, that we can see, than their *position*.

This mode of limitation of the vital affinities by which the selection and appropriation of living matter is effected, is only a statement of fact, and the most general fact that has been ascertained; and it seems highly probable, that it will be found an ultimate fact, in this department of science. It may serve to familiarize our minds with this principle to observe, *first*, that it is precisely analogous to the principle which is now well established as a first truth in the physiology of the nervous system, that portions of nervous matter, precisely similar in structure and composition, have perfectly different endowments according to the anatomical position which they occupy; and, *secondly*, that the same principle seems distinctly exemplified in various cases of diseased action. The phenomena of inflammation, and especially the easy recurrence of inflammation once excited at any one spot in a living animal, indicate that certain vital attractions and affinities existing among the particles of the blood, and between them and the surrounding textures, are peculiarly modified, not merely in a particular manner, but exclusively at a particular spot. From the spot where it commences (*e. g.*, on a serous membrane), this alteration of vital actions extends, as from a centre, to parts that are contiguous to, although having no vascular connection with, that where it commenced, as we see in tracing it from one fold of the peritoneum to another. And when we examine the results of the inflammation in the dead body, we see what clearly shews the operation of a force, producing chemical changes of the kind we are now considering, but acting only at one part, and in one direction. " The capillaries which have taken on the appearance of inflammation, are all on one side of the fine membrane, and the serum and lymph, effusions from these vessels," by which the diseased state is essentially characterized, " are all on the other."—(GOODSIR, *Anatomical and Pathological Observations*, p. 43.)

In saying that the fundamental property of chemical selection, essential to the growth of all living bodies, is strictly a vital property, we do not overlook the fact that various substances, composed of inanimate or inorganic matter, have likewise different powers of attraction for different elements or compounds brought into contact with them. It appears to be only by reference to this property, that we can explain the well-known phenomena of *endosmose* and *exosmose*, in which different fluids, brought in contact with a solid body, are attracted into its pores with very different degrees of force. It is not the nature of the process by which the selection, in the case of the living body, is effected; but the peculiarities of the selections themselves, their great force, and yet uniformly temporary existence, that entitle us to regard them as indicating a vital property.

II. But when we attend to the peculiar changes effected by living solids on the fluid matters which are brought in contact with them, we find that these are by no means confined to the *selection* and appropriation, at particular points, of

compounds pre-existing in that fluid; but that, under the influence of the living solid, *transformations* or new arrangements of the chemical elements take place, and new compounds are formed.

In regard to the precise nature, or seat, of some of these transformations, there is considerable difficulty, but we are at present concerned only with the principle; and may state in illustration of it, two cases of transformation, of which there is no doubt, the change from carbonic acid and water to starch in the cells of plants (oxygen escaping), and the change from starch to fat in the cells of animals (carbonic acid and water escaping). And that I am correct in asserting that the organ which exercises this and other chemical powers in living plants is not only of the simplest construction, but of uniform construction, while the products of its action are very various, will appear from the following statement by MÜLDER.

" Pure cellulose is easily obtained from the pith of the elder-tree, from very young roots, and from other young parts of plants. From these parts it is prepared by digesting them, after being minutely divided, with alcohol, ether, diluted potash, hydrochloric acid, and water. In this manner, the starch, gum, fats, resins, vegetable alkalis, salts, sugar,—and at the same time the peculiar woody matter are separated."

" After the action of these solvents, and especially of the alkali, the cellulose, which was formerly solid and dense, appears in a spongy form. We may state as a fact, that the proper tissue *of all plants* which have been previously exposed to the influence of these solvents, leaves *a substance which is identical in all of them*, a substance which contains carbon and the elements of water."—(*Chemistry of Vegetable and Animal Physiology*, pp. 188–195.)

MÜLDER annexes to this statement a speculation in regard to the influence of *forms* in organized bodies, as affecting their chemical powers or properties, which, so far as I can understand it, I think fitted to convey an erroneous impression.

" One of the first and chief laws visible in organic nature is, that the form has as much influence on the character of the phenomena as the substance of which that form consists. The effects of the primary forces existing in the molecules, have become, by the combination of elements into hollow globules, altogether peculiar."

" In organic nature, besides all the peculiarities existing in the carbon, hydrogen, and oxygen, we must suppose, as a chief consequence of this, a tendency to form membranaceous, concave, spherical little bodies, in which, *because of this form*, new peculiar properties manifest themselves, which cannot be brought out by other forms. Thus, by matter and form, all that we observe in nature is, to a great extent determined."—(*Ibid.*, p. 189.) If by this it is meant that the acquisition of the form is the physical cause of the existence of the properties which cells, or any other organized structures present in the living state, two questions

immediately present themselves, *first*, How are the cells themselves formed (*e. g.* on the germinal membrane of the ovum) out of a matter which is originally without form, otherwise than by those very properties which are here ascribed to their existence? and, *secondly*, If the properties are dependent only on forms, why do they not exist in the dead state, when the forms are, in many instances, still perfect? The enunciation of these questions seems to me sufficient to shew, that the correct expression of the state of our knowledge on this point is that already quoted from LEIBIG, that the chemical forces in living bodies are subject, not simply to an influence of forms, but to "*the invisible cause by which the forms of organs are produced*," *i. e.*, that we must include under the head of vital properties, both the mechanical, or simply attractive power, by which cells or other organs are formed out of amorphous matter, and likewise the chemical powers with which these cells are endowed.

It is no objection to what has been stated, of the strictly vital nature of these chemical powers, to admit that their action is very often *analogous* to the principle to which the name *catalysis* is given by chemists, and which is exemplified likewise in the chemistry of inorganic compounds, where the combination of two substances is determined by the presence of a third, which nevertheless takes no part in the combination itself; or that it is analogous to that disturbance of the equilibrum of chemical compounds, by which the fermentation of an organic compound is transferred to another in contact with it, although the changes in the two go on separately, and the compounds formed are different. It is quite true, that these modes of chemical action resemble and illustrate the manner in which living solids, themselves undergoing continual changes of composition, determine new arrangements of the elements of the compound fluids which are brought in contact with them. But this analogy is far from being an explanation or resolution of the one phenomenon into the other. In the first place, the analogy is essentially defective; because although it is true that in any living being, already existing, different chemical compounds already exist in different parts of the structure, which may act in these modes on the nourishing fluid, and determine distinct transformations of these at different parts; yet this does not apply, as already observed, to the first formation of each of the textures, at its appropriate point, from a homogeneous semi-fluid matter. But farther, although we were to admit the *analogy* of all the chemical processes going on in living beings, to these forms of simply chemical action, we should not thereby be authorised to conclude that the vital processes have not that peculiarity which makes it incumbent on us to regard them as a separate class. We say that the decomposition of carbonic acid, the combination of the carbon with the elements of water to form starch, and the evolution of the oxygen, is a vital action,—not because it is a change different in kind from the decomposition of water and evolution of the hydrogen by iron and acid,—but simply because it indicates an affinity peculiar to the state of life;—because in no other circumstances, when the elements of

water are brought into contact with carbonic acid, is any such decomposition effected. So also, although it is true that the presence of spongy platinum enables oxygen and hydrogen to unite and form water, or the presence of fermenting yeast enables sugar to undergo transformation into carbonic acid and alcohol, still these facts do not interfere with those essential peculiarities on which the doctrine of vital affinity depends, viz., that the presence of living cells composed of carbon and the elements of water, determines both the addition of new matter, from a compound fluid, to those cells, and likewise the formation of other compounds within the cells, varying in different parts of the same structure,—all these compounds being different from any which the chemist can form out of the same elements, and different from those to which the same elements inevitably return, after the phenomena of life are over. The physical principle of catalysis may be said to *illustrate* the transformations in living bodies, as that of endosmose illustrates the selection and appropriation of chemical elements or compounds in living structures; but these principles, as exemplified in dead matter, include none of the peculiarities of the vital chemical actions, and therefore furnish no *explanation* of them.

The materials of which animal bodies are composed, have been now so generally found to have been prepared for them by vegetables, that it has been reasonably doubted, whether any such power of decomposing the fluids presented to them, and forming new compounds, exists in animals. There are some cases, however, in which it appears certain that an action of this kind goes on in living animals, and that it is effected, as in vegetables, by an agency of cells. Thus, there is good evidence that, in the natural state, much of the bile which is discharged into the intestines from the liver is re-absorbed in its passage along the Primæ Viæ; yet it never appears in the chyle, nor, in the natural state, in the blood; which seems to imply that it is decomposed, and its elements thrown into other combinations, in the course of the *cellular action* which attends the absorption of chyle.

In like manner, the formation of fatty compounds out of starch, or its kindred principles, as illustrated by the recent precise observations on the formation of wax by bees, and the formation of gelatine in the living animal, are undoubted instances of chemical transformations thus effected. The precise scene of these transformations is not yet ascertained, but we have strong reason from analogy to suppose that they are effected in the course of the circulation. And as we are certain that the greatest of all the chemical changes which are peculiar to living beings are effected within the cells of vegetables, it seems in the highest degree probable, that the corpuscles or cells (both red and white) which form so large a part of the blood of animals, are concerned in the chemical transformations which take place in blood; and therefore, that we are to regard organized and living cells as the agents or instruments employed by nature in effecting all those chemical changes which are peculiar to the state of life. And if we consider this

principle as established, it goes far to explain several facts, long regarded as obscure, in regard to the structure and position of the lymphatic and lacteal vessels. We know that the mode of origin of these vessels gives *time* and opportunity for cellular action, (*i. e.*, the development, growth, and rupture of cells,) and consequent chemical changes, at their extremities; we know that such cellular action does in fact go on there, particularly in the lacteals; and we know that the substances absorbed there, and probably elsewhere, by these vessels, are in fact altered, and so far assimilated, in the act of absorption; as in the case, already mentioned, of bile absorbed from the intestines. Thus we are led to see the importance of these vessels being placed at all points where substances are to be absorbed, which are foreign to the animal economy, or require chemical change, in order that they may be introduced with safety or good effect. Hence, also, we see the use of the lymphatic glands, at which another opportunity for celluiar action, for chemical changes and assimilation, according to the observations of Mr Goodsir, is provided.* And this also enables us to understand a general fact, which, although disputed, I believe to be both true and important in pathology,—that a substance destined for excretion, but retained in the blood by reason of disease of its excreting gland (particularly the bile or urine), is more injurious than the same matter when secreted by the gland, but re-absorbed from a mucous surface, and consequently subjected to cellular action, and thereby to chemical change.

III. Another general fact appears to be sufficiently illustrated by observations on the chemical changes in living bodies,—viz., That the vital properties by which these are effected are *transferred* from the portions of matter already possessing them, to those other portions of matter which are either taken into their substance, or deposited in their immediate neighbourhood. It is, indeed, obvious, that if we are right in saying that living matter possesses these peculiar vital properties, the act of assimilation which we know to be continually going on in living bodies, is not merely the attraction and addition of new matter, but must include this transference of vital properties to the matter which is continually added to the existing solids.

"The force with which life is kept up," says Professor Whewell, "not only produces motion and chemical change, but also *vitalizes* the matter on which it acts, giving it the power of producing the same changes in other matter, and so on indefinitely. It not only circulates the particles of matter, but puts them in a stream, of which the flow is development as well as movement."—(*Philosophy of Inductive Sciences*, vol. ii., p. 52.)

Several facts which are known in physiology and pathology, may be noticed as more special exemplifications of this principle. Thus, we know that vessels in

* See Carpenter's Manual of Physiology, § 493.

any part of the body communicate certain properties to the whole mass of blood which lies in contact with them, so as to modify or suspend for a long time the coagulation of such blood;—that the blood which enters the vessels of any part where inflammation has been excited, has peculiar properties impressed on it, and even changes on its composition effected, merely by coming in contact with the portions of vessels where that process is going on, and with the portions of blood previously subjected to it;—that the exudation from inflamed vessels acquires peculiar properties from the contact with the living surface on which it lies, first arranging itself as an organized structure, and then selecting and appropriating, from the neighbouring bloodvessels, those materials by which it is assimilated to the texture with which it is connected;—again, that, in the sound state, every portion of matter which is deposited from the bloodvessels, to form part of a muscle or of a nerve, immediately acquires the peculiar vital properties of the part which it nourishes; and, in the case of muscles, even that the change produced in a portion of a fibre by the application of a stimulus, is instantly communicated to the whole length of that fibre, and to many adjoining fibres. It appears to be nearly in the same manner that every portion of carbon and water which enters into the composition of any living vegetable cell, acquires the power of exerting the same vital affinities as actuated the matter which it replaces, or to which it is added.

IV. Another principle, at least equally important and characteristic, may be stated in regard to this communication of vital properties to the materials which are added to living bodies, viz., That such powers are imparted only for a brief period of time, and that long before the time of the death of the structure to which they belong, all those materials lose the vital properties which have been given to them; perhaps, as has been lately stated, as a consequence of the exercise of their peculiar vital powers, perhaps merely as a general law of vitality; but equally, whether the peculiar properties which they acquire in living bodies are of the nature of nervous actions, vital contractions or attractions, or vital affinities. But as this principle is best illustrated by reference to the phenomena of excretions, we delay doing more than merely enunciating it at present.

Having so far considered the general nature of the chemical changes which are peculiar to living bodies, and the kind of apparatus provided by nature for carrying on these changes, we may next take a more special view of the different chemical changes themselves, beginning with the greatest and most fundamental of all, the formation of the amylaceous matters by vegetables, acting on the water and carbonic acid with which they are supplied, both in the liquid form by their roots, and in the gaseous form by their leaves,—and the consequent evolution of oxygen. In regard to this grand function of living plants, the following facts seem the most important that have been ascertained.

1. We see this change effected, in the present order of things, only by the agency of one of the amylaceous principles themselves, although the quantity of that pre-existent matter, in the case of the seeds of many vegetables, is exceedingly minute. We need not enter on the question how far, besides the pre-existence of matter capable of forming cells, in the textures of the plant itself, previously existing organized matter, in the dead state, is essential as part of the nutriment of vegetables,—farther than to observe, that, as the seed of every plant contains a store of organic compounds already formed, there is certainly a strong presumption that a certain quantity of such compounds, formed by previous living processes, is highly useful, if not necessary, to the nourishment of vegetables as well as animals. This, however, appears most important in the early period of the existence of plants, when their power of decomposing the carbonic acid has not yet attained its full intensity. The evidence of the greater part of the nourishment of vegetables being from carbonic acid, water, and ammonia, applied to their leaves, or absorbed by their roots, is quite conclusive; and when we consider that vegetables preceded the appearance of animals on earth, that the first vegetables (as is well observed by LIEBIG) were of the kind which depend least on their roots and most on their leaves for subsistence, and that the kinds of animals which first inhabited the earth, were those which consume the smallest quantity of oxygen, and can live, therefore, in air highly charged with carbonic acid,—it appears in the highest degree probable, that a gradual purification of the atmosphere by the agency of vegetables abstracting carbon, was a necessary prelude to the introduction of animals, especially of warm-blooded animals, into the world: and that the greater part of the carbon now existing in the soil on the earth's surface, originally existed in the form of carbonic acid in the atmosphere, and has been gradually fixed, and enabled to become the chief support of all living beings, by this vital affinity of vegetables, and of those tribes of the lowest marine animals, which have been found to possess the same property, whereby carbon is separated from oxygen, and combined with the elements of water, to form the amylaceous matters.

2. The dependence of the exercise of this property on the presence of light, and its connection (according to the statements of Dr DRAPER), not with the heating portion of the rays, nor with those which effect other chemical changes, but simply with the luminous portion of the rays, shews distinctly that all living action on this globe is equally dependent on light as on heat, although it is, and may long be doubtful, in what manner the influence of light is exerted in producing this change; whether the theory long ago proposed by Sir H. DAVY is admissible, that light enters into the composition of oxygen gas when disengaged from any solid or liquid compound containing it; or whether the agency of light may be better expressed by saying, that it is the necessary stimulus to that kind of vital action which leads to this primary transformation of the elements of which organized beings are composed.

3. It is unnecessary to enter here on the varieties of this amylaceous matter which are formed in different vegetables or parts of the same, the *cellulose* of which the cells are formed, the *starch*, the *dextrin*, the *gum*, the *inuline*, which are deposited in different species and in different parts. All these appear to have the same simple fundamental composition, consisting almost entirely of carbon with the elements of water, and all are formed out of the same compounds and by a vital affinity essentially the same; it may be partly owing to some imperceptible difference in the relative position of the ultimate atoms, partly to differences in the minute quantities of inorganic matter, and of other organic compounds not yet mentioned, which enter into their composition, that so many varieties are found, not only in these compounds themselves, but in the qualities which they present as found in different species of plants, and even in different individuals of the same species. In the case of a graft inserted on the stem of an individual, or even of a species, different from that which furnishes the shoot, we see that the vital affinities of the particles composing the shoot are capable, not only of extracting from the nourishing fluid of the stock all the compounds required for its development, but of imparting to the living textures formed of those compounds which they extract, all those peculiar properties of form, of colour, of smell, of roughness, smoothness, &c., by which species, and even individuals of the same species, are characterized. And when we consider these facts, I apprehend we must admit that, under the influence of the vital affinities which operate in the cells of living vegetables, much more minute differences of compounds are produced, than can be detected and explained by any chemical analysis.

4. An important question here is, whether the carbonic acid of the air is decomposed in the leaves where it is chiefly taken in, the amylaceous compounds immediately formed with the help of water, and the oxygen set at liberty, or whether that acid is taken into the juices of the plant, as we now know that oxygen is into the blood at the lungs, and gradually decomposed there, letting its oxygen escape gradually, and aiding in the formation of different compounds, besides the varieties of starch. That the latter is the more probable supposition may be inferred, partly from the analogy of the action at the lungs of animals, but chiefly from the fact, that a separation of oxygen is equally required for the elaboration, which certainly takes places in vegetables, of other compounds, of the varieties of oil, and of protein, which are chiefly deposited in other parts of their structures.

5. The relations of compounds of this class to sugar demand more special notice. It seems doubtful whether this is ever the first compound formed; it appears in the sap of various plants when the fluids from the soil are ascending and dissolving the starch which had been formed and stored up by the living actions of the preceding year; it appears in almost exactly the same circumstances dur-

ing the germination of seeds, and in both these cases is useful as giving a greater degree of solubility to the starch whence it is formed. In both cases it disappears, and probably is converted into some of the varieties of starch, as the vital actions of the plant become more vigorous. Its composition, in its different varieties, as given by most analysts, $C_{12}\ H_{11}\ O_{11}$, or $C_{10}\ H_{10}\ O_{10}$, or even $C_{12}\ H_{14}\ O_{14}$, denotes that if it be formed from the starch, $C_{12}\ H_{10}\ O_{10}$, it must be either by the addition of the elements of water, or by the abstraction of carbon; and as its formation, during the germination of seeds, is attended with evolution of carbonic acid, it seems most probable that, in that case at least, it is formed in this last way, under the influence of the oxygen of the air. It appears again in the nectaries of flowers, and in the ripening of fruits, as one of the latest results of the vital action of plants, in those parts of them which are fully exposed to air and light, but at a time when we may reasonably suppose that the vital affinities are becoming comparatively ineffective, and when carbonic acid is again evolved. It may be formed by the chemist from some of the varieties of starch by a kind of fermentation, excited by diastase, as in malting; or by a catalytic action of sulphuric acid; and it is formed from starch merely by the agency of cold, as in frozen potatoes, and from inuline merely by continued boiling in water; so that its formation from starch in vegetables seems to be most probably a simple chemical change, not the effect of a vital affinity. Farther, it is a compound which takes the crystalline form, essentially different from any form assumed by those parts of organized structures which exhibit truly vital phenomena, and retains its properties when exposed to air and water better than any of the matters of which organized forms are composed. From all these facts it may be inferred, with great probability, that sugar, as it appears in the living vegetable, is generally to be regarded as a first product of decomposition of starch, by the agency of water and of the oxygen of the air, which appears to be the great agent in the resolution of those compounds, which the vital affinities have built up.

6. On the other hand, the relation of starch and cellulose to the lignin, which forms the greater part of the solid matters of dicotyledonous plants seems to be nearly the reverse of their relation to sugar. This matter is always found incrusting, or incorporated with, the cells of vegetable textures; it gives them their solidity and strength, which all decompositions by chemical agents impair; it cannot be formed from the compounds of starch by artificial means, but is formed from them in greatest quantity when the vital actions of plants are strongest; and its composition is always stated as differing from the amylaceous compounds by containing more carbon; and less oxygen, in proportion to the hydrogen, than exists in the composition of water; its formula being stated as $C_{40}\ H_{23}\ O_{18}$. This, therefore, would appear to be clearly the result of truly vital affinities, continuing to actuate the elements of starch, after the formation of the

starch from carbonic acid and water has been completed, and effecting a decomposition of part of the water, as well as of the carbonic acid, presented to the living vegetable.

In studying this first and most striking of all the changes which are to be ascribed to vital affinities, it is especially necessary to understand the parts assigned to Carbon and Oxygen; and, in taking this general view, we must regard vegetables and animals as inseparably linked together, and look to the whole series of chemical changes which intervene between the origin of vegetables and the death and decomposition of animals. We must regard the carbon, originally existing in combination with oxygen in the atmosphere, in the proportion of one equivalent to two, as the great agent employed by Nature in the formation of the whole organized creation, insomuch that all organic chemistry may be said to be the chemistry of compounds of carbon.—(*Gregory's Chemistry*, p. 241.) That it may fulfil this office it is invested with peculiar but temporary powers; it is separated at particular points and under certain conditions from the oxygen, and attaches itself to the elements of water, always present where vegetables grow, and so forms various compounds, beginning with the varieties of starch; in all which it is the principal ingredient. The compounds thus formed next attack and partially decompose the water and appropriate the hydrogen, thus causing a farther evolution of oxygen, and forming oil; and afterwards nitrogen, in small quantity, is introduced, and fresh transformations take place, by which the protein compounds are formed. All the solid structures of vegetables, and indeed of organized beings generally, are made up of these compounds of carbon, in which oxygen exists either in the proportion to hydrogen which forms water, or in a less proportion than that; and the formation of these may be confidently ascribed to vital affinities. But it is easy to conceive that other compounds of carbon with hydrogen and oxygen will exist in plants in which the oxygen will be in larger proportion than this, without supposing oxygen from the air to be added; because the vital affinities may not have been in sufficient force to separate the oxygen completely from its original union with carbon, and these, therefore, may be regarded as compounds of carbon, water, and undecomposed carbonic acid. Such are the different organic acids (the citric $12\,C\ 8\,H\ 14\,O = 9\,C + 8\,HO + 3\,CO_2$, the malic $8\,C\ 6\,H\ 10\,O = 6\,C + 6\,HO + 2\,CO_2$, the tartaric $8\,C\ 4\,H\ 10\,O = 5\,C + 4\,HO + 3\,CO_2$, the oxalic $4\,C\ 2\,H\ 8\,O = C + 2\,HO + 3\,CO_2$) which are found in the juices of many vegetables, particularly in the immature state.

Again, it is always to be observed, not only that all organized bodies are destined ultimately to revert to the water, carbonic acid, and ammonia, from which they were originally formed, but that, in the case of animals at least, there is a process always going on during the state of life, by which these same inorganic matters are continually evolved from the living frames. Therefore, we cannot be surprised to find that the fluids of all living animal bodies contain other

compounds, in which the characteristic predominance of carbon is not perceived; because they are those which are formed in circumstances where the vital affinities are losing their power, and where a step has been made towards that final dissolution of organic compounds, when the oxygen is to resume its power over the carbon, and this is to revert, directly or indirectly, to the condition of carbonic acid. This general principle as to the respective offices of carbon and oxygen in living bodies,—the one the main agent in nourishing and supporting living structures, the other in maintaining the excretions by which these structures are continually restored to the inorganic world,—we shall find to be applicable, not only to the excretion of carbonic acid and water by the skin and lungs, as compared with the amylaceous compounds taken into animal bodies, but likewise to the excretions by the liver and kidneys, as compared with the two other great constituents of the food of animals, viz., the oily and the albuminous substances.

Oxygen, in its elementary state, although indispensable to all living action, —although a condition of vitality equally universal as heat,—yet hardly enters, if it enters at all, into any of the combinations which are due to the vital affinities. Although taken into the interior of every living being, it appears to comport itself there almost, if not entirely, as it does in acting on dead matter. The expression of LIEBIG, that the action of the oxygen of the air in living bodies is *destructive*, is perhaps fitted to convey an erroneous idea, but we are certain that its chief, if not its sole, action in the animal economy, is on those portions of matter which have no vital properties; either because they are redundant,—not required for the nourishment of the tissues,—or because they have been re-absorbed from them, having lost their vital affinities; and with these it unites, only to carry them off in the excretions, particularly in the great excretion by the lungs. We now know that the speculation as to the connection of the oxygen of the air with vital action, long and ably maintained by the late Mr ELLIS, viz., that its sole use is to dissolve and carry off excreted carbon, and therefore that in the bodies of animals it goes no farther than the lungs, was erroneous; but we may assert with much confidence, that it goes no farther than the circulating blood; and that, although its action there is essential to all the metamorphoses which are there accomplished, yet all the combinations into which it actually enters, are destined to immediate separation from the living body,—being, in fact, the media by which all living bodies, at all periods of their existence, are continually resolving themselves into the inanimate elements from which they sprung. This principle will be better illustrated, however, by a review of the leading facts lately ascertained as to the formation of the other compounds peculiar to organized bodies, and the excretions of animals.

EDINBURGH, *April* 1846.

XVIII.—*Account of some Experiments on the Temperature of the Earth at different Depths, and in different Soils, near Edinburgh.* By James D. Forbes, Esq., F.R.S., Sec. R.S. Ed., &c. Corresponding Member of the Institute of France, and Professor of Natural Philosophy in the University of Edinburgh.

I. *History of the Observations.*

The proper temperature of our globe is a question which, formerly abandoned to speculation and hypothesis, has only lately been made the subject of direct experiment. Preliminary to it, and intimately connected with it, is another inquiry of great interest, namely, What is the thermometric effect of the whole solar heat which falls in a year on the surface of the globe? How much is transmitted to the interior? How much dissipated at the surface? To what depth does the influence of the seasons extend, and in what manner is that influence modified at different depths? It is impossible to say to how many curious and important inquiries a solution of these preliminary questions may lead the way; and it is to them that our attention is at present to be confined. We shall not speak, except incidentally, of the absolute heat of the interior of the globe; we shall only discuss the modifications of the solar heating influence near its surface.

This inquiry was perhaps first agitated by the illustrious Lambert, a mathematical philosopher of Germany, who yields, in originality, in comprehensiveness of mind, and in the successful application of mathematics to a wide range of important physical subjects, to very few of his contemporaries or successors. His experiments were made by M. Ott, a merchant at Zurich, who had a convenient garden for introducing the thermometers.*

Having claimed for Lambert the first systematic inquiry on this subject (for the previous essays of Mariotte and Hales could not lead to any exact conclusions), I shall not trace the subsequent history of the problem, which is fully stated in M. Quetelet's papers, in the Transactions of the Brussels Academy for 1836 and 1840, and in the *Annales de l'Observatoire de Bruxelles*, tome iv. (1845). Such observations were made by Herrenschneider, at Strasbourg; Muncke, at Heidelberg; by Leslie, at Raith, near Edinburgh; by Arago, at Paris; by Quetelet, at Brussels; and by Rudberg, at Upsala. As it does not appear, that, in making or discussing these observations, regard has been had to the influence of

* Lambert, *Pyrometrie*, 4to, 1779, page 356.

a peculiar character of the soil or rock whose temperature was observed, it occurred to me, several years ago, to make several series of observations, under circumstances as exactly similar as possible, with the exception of the nature of the soil or rock. The neighbourhood of Edinburgh, from its variety of geological character, offered peculiar facilities for this purpose; and the British Association, at my request, undertook the expense of providing and inserting thermometers in three different positions, at depths corresponding to those already employed at Brussels, namely, 3, 6, 12, and 24 French feet below the surface. The results have already been partly published in the Proceedings of the British Association, and of the Royal Society of Edinburgh. Deeming it advisable that the curves containing the details of the observations should be published at large, I requested permission from the Committee of Recommendations of the British Association, in 1845, to communicate them, for this purpose, to one of the Royal Societies; and the Council of the Royal Society of Edinburgh having agreed to be at the necessary expense of the plates, I am enabled to present the results in their present complete form, and founded upon five years' observations.

II. LESLIE'S *Observations at Abbotshall, in Fife.*

I shall here reproduce the particulars of the observations of the temperature of the ground at Abbotshall, in Fifeshire, on the property of Raith, close to the town of Kirkcaldy. The distance from Edinburgh is sufficiently small (11 miles in a right line) to render these observations comparable with ours; but I quote them more particularly, because those who have hitherto made use of them, being unaware of the original account published by Sir JOHN LESLIE,* have made almost every possible mistake as to the locality, circumstances, and depths of these observations. It will be seen that they were made by Mr FERGUSON of Raith's gardener. The following extract contains all the important details.

"In order to throw distinct light on a subject so curious and important, ROBERT FERGUSON, Esq. of Raith, a gentleman whose elegant mind is imbued with the love of science, caused, lately, a series of large mercurial thermometers, with stems of unusual length, to be planted in his spacious garden at Abbotshall, about 50 feet above the level of the sea, and nearly a mile from the shore of Kirkcaldy, in latitude 56° 10′. The main part of each stem having a very narrow bore, had a piece of wider tube joined above it; and, to support the internal pressure of the column of mercury, the bulbs were formed of thick cylinders. The instruments, inclosed for protection in wooden cases, were then sunk beside each other to the depths of one, two, four, and eight feet below the surface, in a soft gravelly soil, which turns, at four feet, into quicksand, or a bed of sand and water. These thermometers were carefully observed from time to time by Mr CHARLES NORVAL,

* Supplement to the 6th edition of the Encyclopedia Britannica, *article* CLIMATE, incorporated in the 7th edition.

the very intelligent gardener at Raith; and we have now before us a register of their variations for nearly three years. It thence appears, that, in this climate, and on naked soil, the frost seldom or never penetrates one foot into the ground."

* * * * * *

"These observations are quite satisfactory, and exhibit very clearly the slow progress by which the impressions of heat or cold penetrate into the ground. It will not be far from the truth to estimate the rate of this penetration at an inch every day. The thermometers hence attained their maximum at different periods, though in a tolerably regular succession. The mean temperature of the ground, however, seemed rather to increase with the depth; but this anomaly has evidently proceeded from the coldness of the two last summers, and particularly that of 1816, which occasioned such late harvests and scanty crops. Thus, the thermometer of one foot indicated the medium heat of only 43°·8 during the whole of the year 1816. But it will be satisfactory to exhibit the leading facts in a tabular form. The following are the mean results for each month, only those for December 1817 are supplied from the corresponding month in 1815.

TABLE I. LESLIE'S OBSERVATIONS.

	1816.				1817.			
	1 Foot.	2 Feet.	4 Feet.	8 Feet.	1 Foot.	2 Feet.	4 Feet.	8 Feet.
January	33°·0	36°·3	40°·7	43°·0	35°·6	38°·7	40°·5	45°·1
February	33 ·7	36 ·0	39 ·0	42 ·0	37 ·0	40 ·0	41 ·6	42 ·7
March	35 ·0	36 ·7	39 ·6	42 ·3	39 ·4	40 ·2	41 ·7	42 ·5
April	39 ·7	38 ·4	41 ·4	43 ·8	45 ·0	42 ·4	42 ·6	42 ·6
May	44 ·0	43 ·3	43 ·4	44 ·0	46 ·8	44 ·7	44 ·6	44 ·2
June	51 ·6	50 ·0	47 ·1	45 ·8	51 ·1	49 ·4	47 ·6	47 ·8
July	54 ·0	52 ·5	50 ·4	47 ·7	55 ·2	55 ·0	51 ·4	49 ·6
August	50 ·0	52 ·5	50 ·6	49 ·4	53 ·4	53 ·9	52 0	50 ·0
September	51 ·6	51 ·3	51 ·8	50 ·0	53 ·0	52 ·7	52 ·0	50 ·7
October	47 ·0	49 ·3	49 ·7	49 ·6	45 ·7	49 ·4	49 ·4	49 ·8
November	40 ·8	43 ·8	46 ·3	45 ·6	41 ·0	44 ·7	47 ·0	47 ·6
December	35 ·7	40 ·0	43 ·0	46 ·0	37 ·9	40 ·8	44 ·9	46 ·4
Mean of whole Year	43 ·8	44 ·1	45 ·1	46 ·0	44 ·9	45 ·9	46 ·2	46 ·6

"If the thermometers had been sunk considerably deeper, they would, no doubt, have indicated a mean temperature of 47°·7. Such is the permanent temperature of a copious spring which flows at a short distance, and about the same elevation, from the side of a basaltic or greenstone rock."

I had intended to have engraved the curves of the course of temperature from

1816 to 1821, which I find preserved in the Natural Philosophy Collection; but my inquiries led me to discover that most of these curves had already appeared in the 4th volume of CONSTABLE's Edinburgh Magazine for 1819, where they have remained apparently quite unknown to scientific men, together with the *details* of the observations on which they were founded, from May 1815 to March 1819, and an explanatory article, which I understand was written by Mr GEORGE BUCHANAN, civil-engineer. These observations, particularly at the commencement, were not made with great regularity, sometimes only a single observation being made in a month, at other times as many as eight. Indeed, they have not appeared to me to be worth reprinting at length; but I have condensed into the following Table the information which they contain, shewing the mean monthly temperature, at different depths, for nearly four complete years.

TABLE II. MEAN RESULTS OF LESLIE'S OBSERVATIONS.

1815–19.				
	1 Foot.	2 Feet.	4 Feet.	8 Feet.
January	36°·0	38°·9	41°·0	43°·8
February	35 ·3	36 ·3	40 ·6	42 ·5
March	38 ·1	38 ·6	41 ·0	42 ·1
April	38 ·6	40 ·2	41 ·8	42 ·2
May	45 ·5	45 ·3	45 ·3	44 ·4
June	54 ·5	52 ·4	49 ·1	46 ·9
July	57 ·2	54 ·8	51 ·9	48 ·2
August	54 ·9	54 ·8	52 ·3	50 ·3
September	53 ·3	53 ·0	52 ·1	51 ·0
October	47 ·7	49 ·5	50 ·0	50 ·0
November	42 ·9	45 ·3	47 ·5	48 ·5
December	37 ·5	40 ·8	43 ·8	46 ·3

III. *Construction of the Instruments.*

In commencing the observations at Edinburgh, it was determined that the thermometers should consist of three sets of four each, the lengths increasing in geometrical progression, and the localities being fixed so as to embrace within a small radius, as great a variety of soil as possible. As the deepest thermometers were to be sunk to a depth of 24 French feet (25·6 English), and the portion of the tube, including a column sufficiently long to register the variations of temperature of the fluid which filled the ball (alcohol), must project a foot or two above the surface, the construction, graduation, and depositing of such unwieldy instruments, were attended with no small practical difficulty. The execution of the

whole was entrusted to Mr ADIE, to whose experience and skill I am greatly indebted for the completion of the whole without any material accident.

For the larger thermometers it is of great consequence that as little liquid as possible should be contained in the stem of the instruments, otherwise the apparent expansion of the column will depend greatly upon the variable temperature of the different parts of the stem, as well as on that of the level. This correction being difficult to apply with mathematical exactness, it was desirable to make it as small as possible (although it would be unwise to overlook the correction altogether, as most observers have done). I accordingly had twelve tubes drawn, each of about 26 feet in length; of the external thickness nearly of a common barometer tube (about half-an-inch), but whose internal diameter was nearly capillary. These were carefully examined throughout, by means of a column of mercury passed through them.

The proportional numbers, representing the calibre of the tubes, were entered in a table now before me, corresponding to every foot of their length; and the tubes were numbered, so that the degree of uniformity of any portion could at any time be ascertained. From these tubes twelve lengths were cut from the most uniform parts, amounting altogether to about 144 feet, for the construction of the three sets of thermometers.

It is to be understood that the capillary tube now spoken of was made with a view to reach the surface of the ground, above which the tube should expand into one having degrees of a convenient length. So small, indeed, was the stem compared to the bulb, that a degree of Fahrenheit in the capillary tube would have occupied, in one case, a space not less than 51 inches long. The wide tube to which the scale was attached, had a bore of about $\frac{1}{16}$ inch, and was made long enough to include the expected range of temperature at their respective depths. These ranges were, however, in some cases rather under-rated.

The bulbs were cylindrical, and varied in size from about 6 to 8 inches long, and $1\frac{1}{2}$ or $2\frac{1}{2}$ wide. They were blown at the glass-house separately from the tubes. The deepest thermometers had the largest bulbs and longest degrees, because the required range was less.

From the length and fineness of the tubes much trouble would have been experienced in filling the thermometers in the usual way. The lower end of the cylindric bulb was, therefore, drawn out into a tube, by which the liquid (freshly boiled alcohol, slightly coloured) was admitted, and it was drawn in by the action of a syringe fixed at the extremity of the long stem. Both ends were then closed in the usual way, an expansion being left at the top as in common alcohol thermometers, but most necessary in this case, in order to allow for the changes of temperature to which the instruments were exposed before sinking them in the ground.

The graduation was one of the most delicate parts of the construction. The

instruments were suspended during winter in a staircase, with their bulbs in water, and the temperature of the air surrounding the stems was carefully noted, and a correction applied for any difference between it and that of the water. The staircase was artificially heated through a few degrees, and after being left for a night, a second point was fixed. The temperatures of the water were determined by the mean of three standard thermometers, which agreed extremely closely indeed, when the error of their freezing points was corrected. The first was a standard by TROUGHTON and SIMMS, belonging to and corrected by myself.* The second was a standard constructed by Mr ADIE for the Royal Society of Edinburgh; and the third a standard having very long degrees, constructed by Mr ADIE for his own use. The first pair of observations were made by Mr ADIE *senior* alone, and scratches marked on the tubes at temperatures corresponding to 41°·73 and 50°·77 by the mean of the standard thermometers. To verify these results I made two additional comparisons with the assistance of Mr ALEXANDER ADIE *junior*, at temperatures 41°·97 and 46°·42, which agreed by interpolation remarkably closely with those of the first experiment, considering the difficulties of the observation. In only one case (the 13 feet thermometer for the Experimental Garden), was the difference at all considerable. A mean result was adopted. The length of 1° in the 24 and 12 feet thermometers being from 1 to 2 inches, and divided into 20ths, $\frac{1}{100}$ can be easily read by estimation. In the others the approximation is less.†

IV. *Localities—Sinking of Thermometers.*

Whilst the preparation of the thermometers was going forward, I had holes prepared for inserting them in the positions already fixed on with reference to the geological peculiarities of the soil. These were—

1. In the Observatory enclosure on the Calton Hill, at a height of 350 feet above the sea. The rock is a porphyritic trap, with a somewhat earthy basis, dull and tough fracture. The exact position is a few yards east of the little transit-house. There are also other buildings in the neighbourhood. The ground rises slightly to the east, and falls abruptly to the west at a distance of about 15 yards. The immediate surface is flat, partly covered with grass, partly with gravel.

* Philosophical Transactions, 1836, p. 577.

† More lately Mr ADIE has constructed two sets of thermometers resembling these, one, extending to 24 French feet for Greenwich Observatory; the other, including only the 12 feet thermometer, for Mr CALDECOTT of Trevandrum in India. Both of these sets of instruments were fortunately transported to their destinations without any accident. The graduation of the Greenwich instruments was performed by myself, and a much larger number of points fixed than above described. The result was examined, and the scale determined, by a simple method of graphical projection and interpolation, which led to the most satisfactory results; I should, therefore, recommend this method to others undertaking the same tedious and difficult operation.

2. In the Experimental Garden, adjoining the Royal Botanic Garden at Inverleith, almost exactly 1 mile NW. of the Observatory, and 280 feet lower, being about 70 feet above the sea. The soil here is a remarkably pure sand, resembling sea-sand, extending to a great depth, and including few pebbles of any size. The precise locality was the flat summit of the rising ground, immediately to the south of the large building or show-room in the Garden. The perforation of the sand was exceedingly easy, owing to the uniformity and dryness of the strata. The four thermometers were inserted in three boxes, near to one another, the two shortest thermometers being placed together in the same hole. The *surface* is garden mould, whereas, at the Observatory and Craigleith, it is covered with vegetation (in the former case interrupted by buildings and gravel walks). This circumstance may not be without some effect. Sir JOHN LESLIE has observed* that cold penetrates deeper through bare soil, or compact pavement, than through turf.

3. At Craigleith quarry, 2¼ miles west from the Observatory, in a mass of coal formation sandstone, which has for many years afforded an abundant and durable building material for the city of Edinburgh. The spot chosen was situated in a field 50 yards north of the house called Craigleith Hill, immediately to the east of the quarry, and about 75 yards distant from its north escarpment. The field was under grass during the first two or three years of the observations, afterwards under crop. The height above the sea is about 150 feet. The thermometers were inserted here, as at the Observatory, in one hole, six or seven inches in diameter at the top, and three at the bottom, which it required several weeks to form with boring-irons in the usual manner. When the hole was empty some water always flowed into it, and stood at a certain height, however often removed.

The insertion of the thermometers into the holes required the greatest precaution; the length and flexibility of the stems of the longest exposing them to great risk of casualty. The operation was managed in the following manner. A strong tripod, 12 or 15 feet high, was erected over the hole, and a ladder still longer attached it, so that a man ascending it could command completely the upper part of the instrument. The tube lay in the angle formed by two united pieces of wood, similar to a roof gutter, where it was secured by loops of string. Being raised, with this defence, into an erect position directly over the hole, the loops were successively cut, and the thermometer allowed to slip from the wooden shield, and to sink to the required depth. Dry sand was then poured in to half the depth of the hole. The second thermometer was then similarly planted, and so of the others; the aperture being well closed round the tubes with clay puddle.†

* Encyc. Brit., Article CLIMATE.

† Subsequently (May 1838), a quantity of Roman cement was employed to secure completely the opening of the holes.

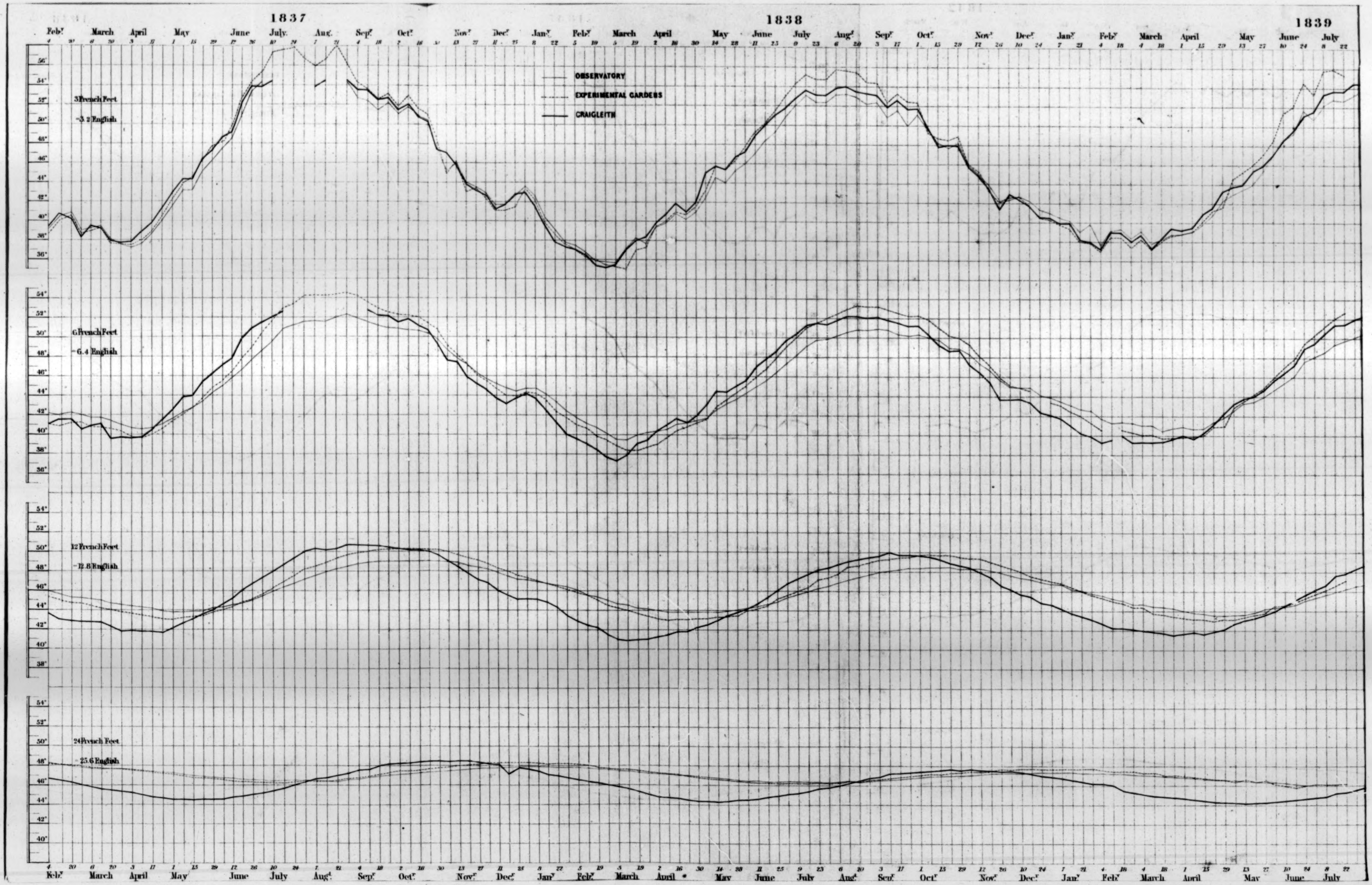
Strong wooden boxes painted green, having doors open to the north side, were then firmly fixed over the projecting stems, which boxes were afterwards pierced with holes, in order to secure a free ventilation. Small thermometers graduated to whole degrees were hung in air within the boxes; and afterwards (May 1838) other thermometers were placed with their bulbs just covered by the soil within the boxes. These last thermometers are referred to in the tables as t_5. After the instruments were finally placed, slight metal scales were attached to the respective tubes with fine copper wire. I should add that, for the defence of the bulbs and the capillary tubes, at their inferior extremities, before mentioned, they were half inserted into tin cylindrical boxes filled up with plaster of Paris.

I had hoped to have commenced the observations with 1st January 1837; the unexpected difficulty experienced in boring the holes, and subsequently the severe weather, prevented the insertion of the thermometers before the 18th January in the Experimental Garden, the 20th on the Calton Hill, and the 21st at Craigleith Quarry; the whole was happily accomplished without the slightest accident. In all my arrangements, I was aided by the civility of the Directors of the Astronomical Institution and of the Experimental Garden, by the astronomer Mr Henderson, and other official persons.

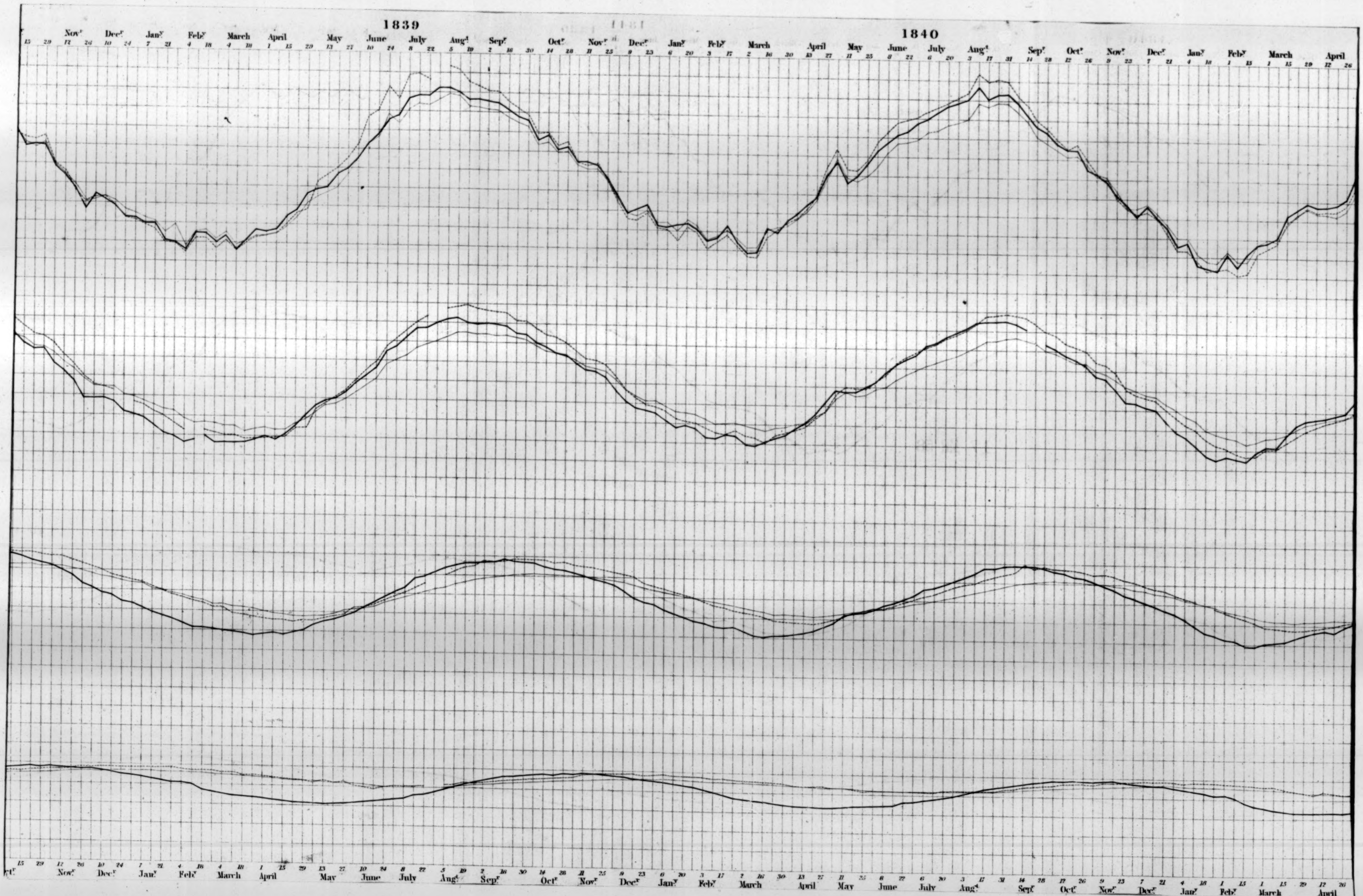
V. *Observations and Observers.*

Not the least of the difficulties of carrying on such observations as the present has ever been found to be, that of getting perfectly trustworthy and zealous observers. In this matter I esteem myself particularly fortunate. Professor Henderson undertook, in the kindest manner, the personal superintendence of the thermometer placed on the Calton Hill in the Observatory grounds. At the Experimental Garden I received the services of Mr James M'Nab, the superintendent; and at Craigleith, those of Mr Mackintosh, whose official connection with the quarry ensures his constant residence. The observations were made weekly, and were registered in degrees and hundredths of Fahrenheit's scale (by approximation). The general superintendence which I have been able to give, assured me that all the observations were made, not only with fidelity, but without any sensible error arising from want of familiarity of two of the observers with instruments so minutely divided. And, were other proofs wanting, fortunately the ultimate projection and comparison of the three sets of observations affords the most perfect check upon any considerable inaccuracy, either in the observations or computations.

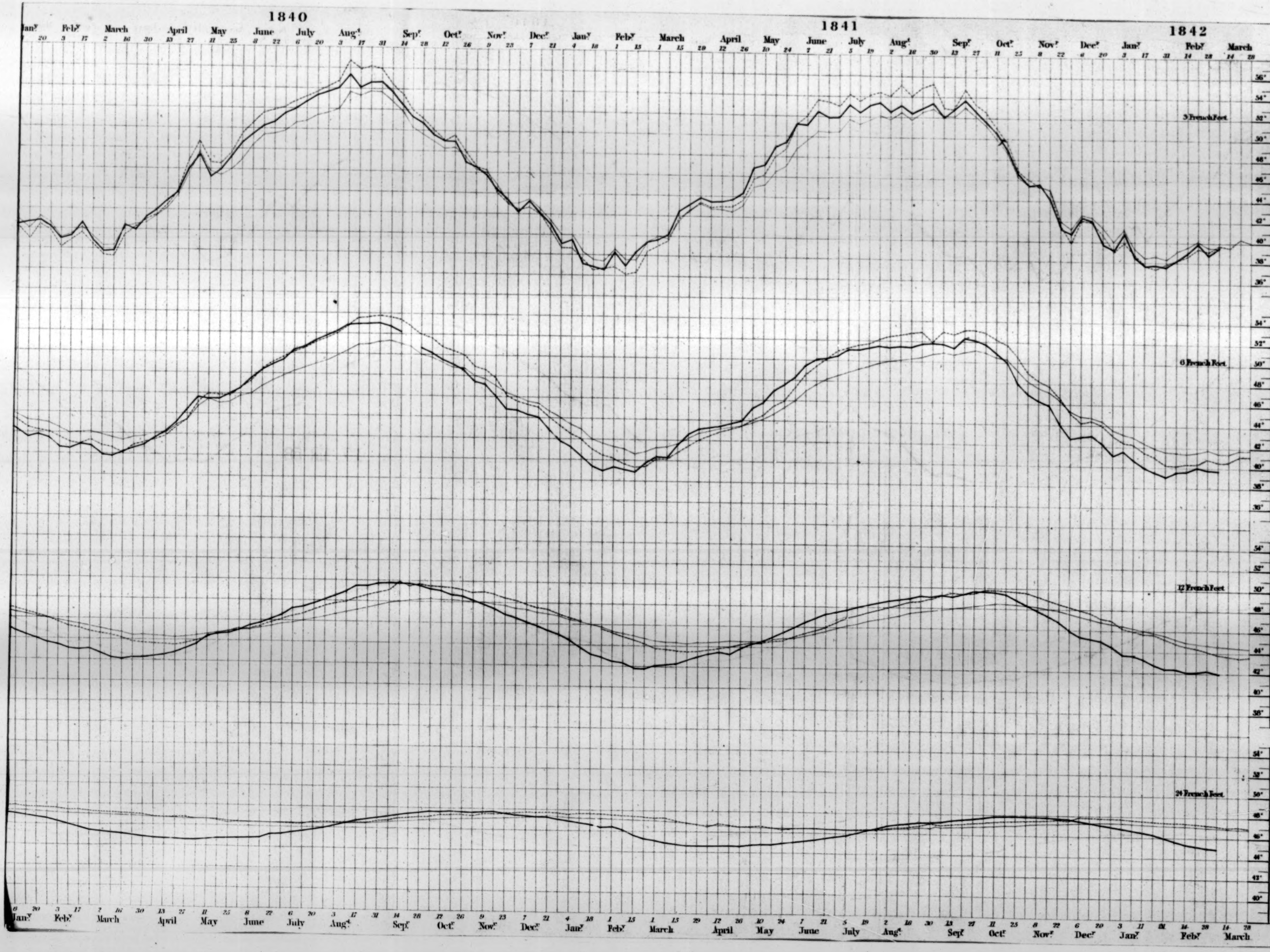
With a view to check any permanent change in the reading of the instruments, such as might arise from a permanent displacement of the freezing point, I had a spirit thermometer constructed with a bulb similar to those buried, with



1837
1838
1839
OBSERVATORY
EXPERIMENTAL GARDENS
CRAIGLEITH
3 French Feet
= 3.2 English
6 French Feet
= 6.4 English
12 French Feet
= 12.8 English
24 French Feet
= 25.6 English



1839
1840
Novr Decr Jany Feby March April May June July Augt Sepr Octr Novr Decr Jany Feby March April May June July Augt Sepr Octr Novr Decr Jany Feby March April



1840
1841
1842
3 French Feet
6 French Feet
12 French Feet
24 French Feet

a view to its occasional verification; the freezing point being accurately ascertained. Nine years after (January 1846), this instrument being re-examined, shewed no appreciable change in the position of the zero point. It could not have amounted to $\frac{1}{20}$ of a degree. A common thermometer would have altered appreciably under similar circumstances. Is not this owing to the strength of the glass bulb?

The permanent influence of the pressure of sand in the holes was suggested to me as a possible source of a change of figure of the instruments; but the conclusive experiments lately made in America and France on the pressure of sand, convinced me that this could have no appreciable influence.

The observations were made weekly; generally on Mondays. As it was commonly supposed that the diurnal variations of the temperature disappear at the depth of three feet, I did not take particular precautions to have all the observations made at the same hour. I find, however, that, at the Observatory, the readings were taken immediately after noon, at the Experimental Garden, about 2 o'clock, at Craigleith Quarry, regularly between 11 and 12 o'clock. The later hour of the second series may account for some irregularities observable. The observations, at all the stations, were made regularly from February 1837 down to May 1842, about which time, the thermometers at Craigleith were maliciously destroyed; but these five years of complete observations have yielded all the results which were looked for in commencing them. The boxes covering the thermometers, in the Experimental Garden, were blown over in the winter 1844–5, crushing the thermometers. Those at the Observatory still remain in good order, and are regularly observed. The numbers, from the original registers, are exactly given in the table at end of this paper, together with the corrections applied to them, in the manner to be described in the next section. It is not to be supposed that the registers are without some errors, at least, in the case of the two less experienced observers (at Experimental Garden and Craigleith); but they are only oversights of the eye or hand, which can affect none of the conclusions, as the admirable coincidence of the three independent series, in Plate VII., sufficiently proves.

The following table contains the data necessary to be known, respecting the scales and dimensions of the thermometers, for correcting the temperature of the stems and exposed columns in the manner which we shall next proceed to investigate.

TABLE III. THERMOMETRIC CONSTANTS.

No.	Range of Wide Tube.	Length of 1° in inches.	Inches of Fine Tube above ground.	Inches of Wide Tube in Fine Tube.	Degrees in Fine Tube.	Length of Fine Tube in inches.	Length of 1° in inches of Fine Tube.	Degrees of Fine Tube sunk below ground.	Degree from which the column exposed to external temperature is reckoned.*
					OBSERVATORY.				
1	41°·6—51°·0	1·79	9	24·5	13°·7	316	23	13°·3	41°
2	41·0—51·0	1·57	9	7·0	4·5	162	36	4·2	40·5
3	36·0—54·0	0·88	9½	4·2	4·8	86	18	4·3	35
4	30·5—55·0	0·63	9½	3·6	5·7	48	8·4	4·6	28·5
					EXPERIMENTAL GARDEN.				
1	39·5—52·9	1.225	8	30·0	24·5	315	12·8	23·9	38
2	38·5—53·2	1·09	6½	9·2	8·4	160	19	8·1	38
3	35·0—55·9	0·68	10	4·2	6·2	87	14	5·5	33·5
4	26·5—60·0	0·475	10	1·8	3·8	48	12·6	3·0	25
					CRAIGLEITH.				
1	40·5—51·5	1·575	9	19·7	12·5	316	25·2	12·1	40
2	40·7—50·7	1·52	8½	4·9	3·2	162	51	3·0	40
3	37·0—53·6	0·98	10	6·4	6·5	87	13·4	5·7	35.5
4	31·5—55·4	0·67	10	1·4	2·1	48	22·8	1·7	31

* This supposes the Atmospheric Temperature to penetrate 9 inches. The tubes at the surface would have the following readings:—

41·2 40·7 35·5 29·4 | 38·9 38·2 34·3 25·7 | 40·1 40·5 36·2 31·1

May 14. 1834. No. 4. Observatory was lowered 3°·12, by withdrawing alcohol. May 15, No. 4. Craigleith lowered by 1°.95. Corrected scales were immediately applied.

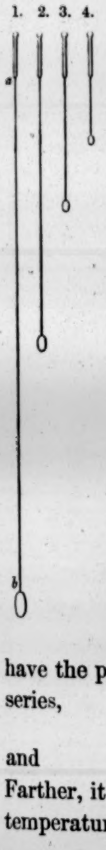
In the above Table, No. 1 is the *longest*, No. 4 the *shortest*, Thermometer.

VI. *Corrections of the Observations.*

It is very evident that the readings of the thermometers cannot indicate exactly the temperature of the point corresponding to the centre of the bulb, because the stem between that point and the surface of the ground never has a uniform temperature throughout; and the portion of the column above ground is affected by the temperature of the air at the moment. Of these two corrections in *our* thermometers, the latter is by much the most important, which is fortunate, because it is also determined with the greatest accuracy.

These corrections, however obvious, have, according to M. QUETELET, been

overlooked by all observers previous to himself and M. Arago. It seems that M. Arago ascertains the expansion of the buried column of spirit by sinking, alongside of each thermometer, a stem similar to its own, having a scale above ground, but no bulb: the variations of bulk of the contained spirit being thus directly shewn are eliminable from the readings of the adjacent thermometer. As far as regards the correction due to the portion of the stem buried in the earth, this mode of correction is ingenious and satisfactory; but, when the tubes are capillary, the reduction is so small that it may readily be obtained otherwise, with sufficient accuracy. It does not, however, apply to the portion of the scale above ground, since the quantity of alcohol, so exposed, varies with the degree shewn by the thermometer. And this correction, as has been said, is, in our observations, the more important of the two. The method which I propose to employ is the following:—



The distribution of the thermometers, in geometrical progression, enables us to employ the temperatures indicated by the thermometers, Nos. 2, 3, and 4, for the correction of the reading of No. 1 (that is, to reduce the temperature of the column $a\,b$ to the temperature of the bulb); the temperature of Nos. 3 and 4 to correct No. 2; and of No. 4 to correct No. 3. It was matter of consideration (1.), how this might be most correctly done; and (2.), to select a formula of sufficient (not superfluous) accuracy, and adapted to calculation.

The mode of doing this was partly arbitrary, and justified by application to cases where the variation of temperature in the stems was a maximum selected from the journals of observation. Thus, the depth of the successive thermometers being—

$$0, \quad a \quad , \quad 2\,a \quad , \quad 2^2\,a \quad , \quad 2^3\,a \quad , \quad \&c.$$

The intervals of depth are—

$$a \qquad a \qquad 2\,a \qquad 2^2\,a \qquad \&c.$$

And the product of the temperature and depth must lie between two series, one of which supposes the temperature of any interval equal to the temperature of the thermometer at its superior limit, the other supposes the mean temperature equal to that at its inferior limit. It is evident that the truth must lie between these suppositions, or that denoting by T, the superficial temperature; and t_4, t_3, t_2, t_1, the indications of each thermometer successively in descending, we must have the product of the temperature and depth between the values of the two series,

$$T\cdot a + t_4\,a + 2\,t_3\,a + 2^2\,t_2\,a$$

and

$$t_4\,a + \ t_3\,a + 2\ t_2\,a + 2^2\,t_1\,a$$

Farther, it will be nearer the latter result than the former, since the variation of temperature diminishes as we descend.

Until after May 1838, no superficial thermometer (or one in the uppermost stratum of soil) was used. There was simply a thermometer suspended beside the scales in the box, to indicate the temperature of the part of the column exposed to the air. Fortunately, however, the correction for the temperature of the first interval a, or 3 feet, is very small indeed. The extreme excess of temperature of the air in the box above the highest thermometer, or $(T-t_4)$ during 1837, was 20° Fahr. In the most capacious of the tubes employed, supposing that the temperature of 20° had been applied through the whole depth of 3 feet, an expansion would have been produced, which would have raised the alcohol on the scale of that thermometer by 0°·07; but the expansion could not possibly amount to half of this, seeing that the mean temperature of the 3 feet of soil would more nearly approach to that of the inferior limit of it, than to that of the air in contact with its surface. We can hardly err ·01 (a quantity in this particular case much less than the errors of reading), by assuming that $\frac{1}{4}$ of the column of 3 feet had the temperature of the air, and the remainder that of the thermometer bulb itself.* In both the other three-feet thermometers, the error, owing to the smaller capacity of the three-feet capillary tube, would be but about half as great.

Now, by reasoning by the method of limits as above, and applying the above correction to the upper 9 inches of all the tubes, I find the following formula to be a more than sufficient approximation in every case. For the deepest thermometer, No. 1, whose temperature is t_1

$$\text{Mean temperature of stem} = \frac{\Sigma \text{ Temperatures} \times \text{depths}}{\text{depth}}.$$

making 3·2 feet = unit of depth,

$$\text{mean temperature} \quad = \frac{1 \cdot t_4 + 2 \cdot t_3 + 4 \cdot t_2 + 8 \cdot t_1}{8}. \quad . \quad . \quad . \quad . \quad (1.)$$

And as the reduction of the temperature of the stem to that of the bulb depends on the excess of the former above the latter, we have

$$\text{Mean excess of temperature} = \frac{(t_4-t_1)+2(t_3-t_1)+4(t_2-t_1)}{8}. \quad . \quad (2.)$$

Farther, to adapt this to calculation, let the successive intervals of temperature of the series of thermometers be taken, and make

$$t_2-t_1=a$$
$$t_3-t_2=b$$
$$t_4-t_3=c$$

the above expression becomes

$$\tfrac{1}{8}\{(a+b+c)+2(a+b)+4a\}$$
$$=\tfrac{1}{8}\{7\,a+3\,b+c\} \text{ for thermometer No. 1,} \quad . \quad . \quad (3.)$$

* The application of this correction becomes exceedingly easy, by considering the correction for air temperature to apply, not only to the exposed part of the tube, but also to the first 9 inches of the buried stem.

For the other thermometers, we have only to make a and b successively $= 0$, and substitute 4 and 2 for the depths, which give

$$\tfrac{1}{4}\{3\,b+c\} \text{ for No. 2.}$$

$$\tfrac{1}{2}\,c \text{ for No. 3.}$$

And the correction for No. 4 will be exclusively that derived from the observation of the thermometer in air T, and has for its argument

$$\tfrac{1}{4}\{\mathrm{T}-t_4\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

I should have observed, that, in order to ascertain that these formulæ represented the state of the instruments with sufficient accuracy, I first calculated how nearly the mean temperature of the whole column of each thermometer must be known, in order to entail no greater error than that of the reading, say of ·01 degree. This, in the case of the deepest (26 feet) thermometer, with the widest bore, amounts to 1° of temperature, and in the three-feet thermometer to 3°.

For the second or Air Temperature correction, the quantity of alcohol to be expanded, depends on the height at which the liquid stands in the tube, and the amount of expansion on the temperature to which it is subjected.

Let us suppose, that in any thermometer the degree of temperature is known at which the surface of the column would just contract below the level of the soil. The number of degrees above this, which the thermometer at any time marks, points out the quantity to be corrected for expansion. If this correction is also to be applied to a part of the tube 9 inches lower, we have only to start from a degree of the scale corresponding to that point instead of to the surface. The number of degrees for which it is to be corrected, is the excess of the temperature of the air above that of the bulb, or $\mathrm{T}-t_n$, t_n denoting the temperature shewn by the n^{th}. thermometer in an ascending order. Table III. in page 198, gives the point on the scale of each thermometer, corresponding to a position 9 inches below the level of the soil; let that point be l_1, l_2, l_3, l_4, for each thermometer in succession, then the number of degrees of temperature to be corrected for, will be $\mathrm{T}-l_1$, $\mathrm{T}-l_2$, &c.

Thus, both the corrections required to reduce the observed readings amount to finding by a table, the increased (or diminished) length of a given column of alcohol (measured in degrees), for a given excess (or defect) of temperature, assigned in degrees. Such a table I have constructed, and I have thought it advisable to employ the correct value of the expansion of alcohol at atmospheric temperatures, instead of its *mean* amount between the freezing and boiling points. This latter quantity as given by DALTON, and commonly employed, is ·11 of the volume, from 32° to 212°, or ·000611 for 1° Fahr. Now, it appears from MUNCKE'S elaborate experiments, that alcohol, of density ·808, expands at common atmospheric temperatures (viz. between 0° and 20° cent.), almost precisely ·001 of its volume

at freezing for 1°. cent.,* or ·000555 for 1° Fahr., a quantity *one-tenth* greater than its mean dilatation usually adopted. I accordingly had a Table constructed on this basis, with a double entry, one for the number of degrees, or space in the tube filled with the expanded liquid, and the other for the excess of temperature to which it is exposed. The arguments used for the two corrections, with this Table, were the following :—

	1st Argument. Capacity in Degrees to be corrected.	2d Argument. Degrees of Temperature to be corrected for
1*st* Correction (for Temperature of Stem),	Table, p. 198.	$\frac{7a+3b+c}{8}$
2*d* Correction (for Temperature of Air), .	$T-t_1$	$T-t_1$

The sign of the correction is always *opposite to that of the second argument.*

* FECHNER's Repertorium, II. 430. The expansion of absolute alcohol is somewhat greater as given by MUNCKE, in a paper in the Petersburg Transactions, read 5th September 1834.

TABLE X. *Corrections in Degrees for the Expansion of Alcohol in Glass Tubes* = ·000555 *for* 1° *Fahr.*

TABLE X. *Corrections in Degrees for the Expansion of Alcohol in Glass Tubes = ·000555 for 1° Fahr.*

Excess of Temp.	DEGREES OF TUBE TO BE CORRECTED.																													
	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	11°	12	13°	14°	15°	16°	17°	18°	19°	20°	21°	22°	23°	24°	25°	26°	27°	28°	29°	30°
1°	·00	·00	·00	·00	·00	·00	·00	·00	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·01	·02	·02	·02	·02
2°	·00	·00	·00	·00	·01	·01	·01	·01	·01	·01	·01	·01	·01	·02	·02	·02	·02	·02	·02	·02	·02	·02	·03	·03	·03	·03	·03	·03	·03	·03
3°	·00	·00	·01	·01	·01	·01	·01	·01	·02	·02	·02	·02	·02	·02	·02	·03	·03	·03	·03	·03	·04	·04	·04	·04	·04	·04	·05	·05	·05	·05
4°	·00	·00	·01	·01	·01	·01	·02	·02	·02	·02	·02	·03	·03	·03	·03	·04	·04	·04	·04	·04	·05	·05	·05	·05	·06	·06	·06	·06	·06	·07
5°	·00	·01	·01	·01	·01	·02	·02	·02	·03	·03	·03	·03	·04	·04	·04	·04	·05	·05	·05	·06	·06	·06	·06	·07	·07	·07	·08	·08	·08	·08
6°	·00	·01	·01	·01	·02	·02	·02	·03	·03	·03	·04	·04	·04	·05	·05	·05	·06	·06	·06	·07	·07	·07	·08	·08	·08	·09	·09	·09	·10	·10
7°	·00	·01	·01	·02	·02	·02	·03	·03	·04	·04	·04	·05	·05	·05	·06	·06	·07	·07	·07	·08	·08	·09	·09	·09	·10	·10	·11	·11	·11	·12
8°	·00	·01	·01	·02	·02	·03	·03	·04	·04	·04	·05	·05	·06	·06	·07	·07	·08	·08	·08	·09	·09	·10	·10	·11	·11	·12	·12	·12	·13	·13
9°	·01	·01	·02	·02	·03	·03	·03	·04	·05	·05	·06	·06	·07	·07	·08	·08	·09	·09	·10	·10	·11	·11	·12	·12	·13	·13	·14	·14	·15	·15
10°	·01	·01	·02	·02	·03	·03	·04	·04	·05	·06	·06	·07	·07	·08	·08	·09	·10	·10	·11	·11	·12	·12	·13	·13	·14	·14	·15	·16	·16	·17
11°	·01	·01	·02	·02	·03	·04	·04	·05	·06	·06	·07	·07	·08	·09	·09	·10	·10	·11	·12	·12	·13	·13	·14	·15	·15	·16	·17	·17	·18	·18
12°	·01	·01	·02	·03	·03	·04	·05	·05	·06	·07	·07	·08	·09	·09	·10	·11	·11	·12	·13	·13	·14	·15	·15	·16	·17	·17	·18	·19	·19	·20
13°	·01	·01	·02	·03	·04	·04	·05	·06	·07	·07	·08	·09	·09	·10	·11	·12	·12	·13	·14	·14	·15	·16	·17	·17	·18	·19	·20	·20	·21	·22
14°	·01	·02	·02	·03	·04	·05	·05	·06	·07	·08	·09	·09	·10	·11	·12	·13	·13	·14	·15	·16	·16	·17	·18	·19	·19	·20	·21	·22	·23	·23
15°	·01	·02	·03	·03	·04	·05	·06	·07	·08	·08	·09	·10	·11	·12	·12	·13	·14	·15	·16	·17	·18	·18	·19	·20	·21	·22	·23	·23	·24	·25
16°	·01	·02	·03	·04	·04	·05	·06	·07	·08	·09	·10	·11	·12	·12	·13	·14	·15	·16	·17	·18	·19	·20	·20	·21	·22	·23	·24	·25	·26	·27
17°	·01	·02	·03	·04	·05	·06	·07	·08	·09	·09	·10	·11	·12	·13	·14	·15	·16	·17	·18	·19	·20	·21	·22	·23	·24	·25	·26	·26	·27	·28
18°	·01	·02	·03	·04	·05	·06	·07	·08	·09	·10	·11	·12	·13	·14	·15	·16	·17	·18	·19	·20	·21	·22	·23	·24	·25	·26	·27	·28	·29	·30
19°	·01	·02	·03	·04	·05	·06	·07	·08	·10	·11	·12	·13	·14	·15	·16	·17	·18	·19	·20	·21	·22	·23	·24	·25	·26	·27	·29	·30	·31	·32
20°	·01	·02	·03	·04	·06	·07	·08	·09	·10	·11	·12	·13	·14	·16	·17	·18	·19	·20	·21	·22	·23	·24	·26	·27	·28	·29	·30	·31	·32	·33
21°	·01	·02	·03	·05	·06	·07	·08	·09	·11	·12	·13	·14	·15	·16	·18	·19	·20	·21	·22	·23	·25	·26	·27	·28	·29	·30	·32	·33	·34	·35
22°	·01	·02	·04	·05	·06	·07	·09	·10	·11	·12	·13	·15	·16	·17	·18	·20	·21	·22	·23	·24	·26	·27	·28	·29	·31	·32	·33	·34	·36	·37
23°	·01	·03	·04	·05	·06	·08	·09	·10	·12	·13	·14	·15	·17	·18	·19	·20	·22	·23	·24	·26	·27	·28	·29	·31	·32	·33	·35	·36	·37	·38
24°	·01	·03	·04	·05	·07	·08	·09	·11	·12	·13	·15	·16	·17	·19	·20	·21	·23	·24	·25	·27	·28	·29	·31	·32	·33	·35	·36	·37	·39	·40
25°	·01	·03	·04	·06	·07	·08	·10	·11	·13	·14	·15	·17	·18	·19	·21	·22	·24	·25	·26	·28	·29	·31	·32	·33	·35	·36	·38	·39	·40	·42
26°	·01	·03	·04	·06	·07	·09	·10	·12	·13	·14	·16	·17	·19	·20	·22	·23	·25	·26	·27	·29	·30	·32	·33	·35	·36	·38	·39	·40	·42	·43
27°	·02	·03	·05	·06	·07	·09	·11	·12	·14	·15	·17	·18	·20	·21	·23	·24	·26	·27	·29	·30	·32	·33	·35	·36	·38	·39	·41	·42	·44	·45
28°	·02	·03	·05	·06	·08	·09	·11	·12	·14	·16	·17	·19	·20	·22	·23	·25	·26	·28	·30	·31	·33	·34	·36	·37	·39	·40	·42	·44	·45	·47
29°	·02	·03	·05	·06	·08	·10	·11	·13	·15	·16	·18	·19	·21	·23	·24	·26	·27	·29	·31	·32	·34	·35	·37	·39	·40	·42	·44	·45	·47	·48
30°	·02	·03	·05	·07	·08	·10	·12	·13	·15	·17	·18	·20	·22	·23	·25	·27	·28	·30	·32	·33	·35	·37	·38	·40	·42	·43	·45	·47	·48	·50
31°	·02	·04	·05	·07	·09	·11	·12	·14	·16	·17	·19	·21	·22	·24	·26	·27	·29	·31	·33	·35	·36	·38	·40	·42	·43	·45	·47	·49	·50	·52
32°	·02	·04	·05	·07	·09	·11	·12	·14	·16	·18	·20	·21	·23	·25	·27	·28	·30	·32	·34	·36	·37	·39	·41	·43	·44	·46	·48	·50	·52	·53
33°	·02	·04	·05	·08	·09	·11	·13	·15	·17	·19	·21	·22	·24	·26	·28	·29	·31	·33	·35	·37	·39	·41	·42	·44	·46	·48	·50	·52	·54	·55
34°	·02	·04	·05	·08	·09	·11	·13	·15	·17	·19	·21	·23	·25	·26	·28	·30	·32	·34	·36	·38	·40	·42	·43	·45	·47	·49	·51	·53	·55	·57
35°	·02	·04	·06	·08	·10	·12	·14	·16	·18	·20	·22	·24	·26	·27	·29	·31	·33	·35	·37	·39	·41	·43	·45	·47	·49	·51	·53	·55	·57	·59
36°	·02	·04	·06	·08	·10	·12	·14	·16	·18	·20	·22	·24	·26	·28	·30	·32	·34	·36	·38	·40	·42	·44	·46	·48	·50	·52	·54	·56	·58	·60
37°	·02	·04	·06	·08	·11	·13	·15	·17	·19	·21	·23	·24	·27	·29	·31	·33	·35	·37	·39	·41	·43	·45	·47	·50	·52	·54	·56	·58	·60	·62
38°	·02	·04	·06	·08	·11	·13	·15	·17	·19	·21	·23	·25	·27	·30	·32	·34	·36	·38	·40	·42	·44	·46	·48	·51	·53	·55	·57	·59	·61	·63
39°	·02	·04	·07	·09	·11	·13	·15	·18	·20	·22	·24	·26	·28	·31	·33	·35	·37	·39	·41	·43	·46	·48	·50	·52	·55	·57	·59	·61	·63	·65
40°	·02	·04	·07	·09	·11	·13	·15	·18	·20	·22	·24	·27	·29	·31	·33	·36	·38	·40	·42	·44	·47	·49	·51	·53	·56	·58	·60	·62	·64	·67

VII. *Results of the Observations.*

A. *Mean Temperature at different depths.*

It has been observed at Brussels and elsewhere,* that, even at depths less than 25 feet, the mean annual temperature indicated by the lower thermometers is *greater* than that of those nearer the surface. This appears to be also clearly established by the observations at the three stations near Edinburgh, as contained in the following Table.

TABLE V. MEAN TEMPERATURE FOR FIVE YEARS.

	3 French Feet, (No. 4).			6 French Feet, (No. 3).			12 French Feet, (No. 2.)			24 French Feet, (No. 1).		
	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.
	°	°	°	°	°	°	°	°	°	°	°	°
1837-1838	45·94	46·54	46·26	46·23	46·70	46·50	46·65	46·90	46·30	47·22	47·28	46·46
1838-1839	44·81	45·44	45·22	45·35	45·90	45·35	46·12	46·44	45·47	46·89	47·05	45·97
1839-1840	45·48	46·14	45·83	45·75	46·43	45·73	46·18	46·67	45·70	46·68	46·98	45·87
1840-1841	45·56	46·26	45·97	45·99	46·58	46·01	46·43	46·91	46·08	46·77	47·04	46·18
1841-1842	45·66	46·29	46·13	45·99	46·51	46·01	46·44	46·89	46·07	46·79	47·08	45·87
Means	45·49	46·13	45·88	45·86	46·42	45·92	46·36	46·76	45·92	46·87	47·09	46·07

Mean of Observatory, ...	46°·14	Elevation, . .	350 feet.
——— Experimental Garden, . . .	46·60	„ . .	70 feet.
——— Craigleith, ...	45·95	„ . .	150 feet.
Mean Temperature of Air from Mr Adie's Observations,	45·28	„ . .	240 feet.

Mean of 3	Feet Thermometers, . .	45°·83
„ 6	„	46·07
„ 12	„	46·36
„ 24	„	46·68

The cause of the increased mean temperature below, is by no means clear. From its irregularity, it is most probably due to several causes, of which the central heat of the earth is perhaps one; its effect at 25 feet need not be insignificant, since the average rate of increase at great depths is 1° Fahr. for from 40 to 50 English feet. In the present case, the increase is not uniform, and it is also decidedly different in the different soils, and will appear by the sequel to be intimately connected with the conducting power of the strata. The order of magnitude of the increase is this, 1. Observatory, 2. Experimental Garden, 3. Craigleith; —or, Trap, Sand, Sandstone, which is also the order of the conducting power. The following table shews that, in every instance but one (from 6 feet to 12 feet at Craigleith), the increase was apparent.

* QUETELET, Annales de l'Observatoire Royale de Bruxelles, iv. 150.

Table VI. Shewing the Rate of Increase of Temperature with Depth.

	Observatory.	Experimental Garden.	Craigleith.
From 3 to 6 feet,	+ 0°·37	+ 0°·29	+ 0°·04
... 3 to 12 ...	+ 0·87	+ 0·63	+ 0·04
... 3 to 24 ...	+ 0·38	+ 0·96	+ 0·19

To complete the comparison of Meteorological data, I subjoin Mr Adie's observations on the mean temperature of the air and the quantity of rain fallen during the same period, at Canaan Cottage, near Edinburgh, 240 feet above the sea, excepting the months previous to May 1838, which were observed on the Regent Terrace, Calton Hill, at about the same elevation above the sea.

Table VII. Mean Temperature and Quantity of Rain for the Years 1837-38-39-40-41-42, observed at Edinburgh by Mr Adie.

	1837.			1838.			1839.	
	Thermometer.	Rain.		Thermometer.	Rain.		Thermometer.	Rain.
January	36 33	1·23	January	31·73	2·47	January	33·05	1·76
February	37·23	2·14	February	30·06	1·21	February	38·53	1·45
March	35·24	1·28	March	38·12	2·76	March	36·98	1·47
April	39·65	1·61	April	40·25	1·78	April	42·53	0·33
May	48·37	1·53	May	44·87	2·90	May	46·82	0·47
June	57·30	2·86	June	53·98	5·16	June	53·42	3·91
July	60·42	4·54	July	58·94	2 45	July	57·77	3·51
August	51·77	4·13	August	56·88	2.99	August	55·51	1·77
September	53·18	1·73	September	52·04	4·00	September	52·13	3·09
October	50·17	2.02	October	46·27	1·15	October	46·50	2·38
November	40·45	2.03	November	38·38	3·06	November	43·13	1·65
December	42·68	1·67	December	38·17	0·73	December	37·46	1·66
Sums	552·79	26 77		529·69	30·86		543·83	23·45
Mean	46·07			44·14			45·32	

	1840.			1841.			1842.	
	Thermometer.	Rain.		Thermometer.	Rain.		Thermometer.	Rain.
January	38·74	3·72	January	33·00	1·23	January	35·45	1·01
February	36·55	1·58	February	38·39	1·66	February	39·55	1·11
March	42·74	0·43	March	45·62	0·60	March	42·04	3·44
April	48·16	0·19	April	44·26	1·14	April	45·03	0·15
May	47·13	3·99	May	51·74	1·14	May	51·22	1·45
June	52·53	2·51	June	52·43	1·36	June	57·53	0·97
July	52·75	3·46	July	53·58	3·87	July		
August	44·51	1·99	August	53·88	3·64	August		
September	48·57	2·39	September	54·36	2·63	September		
October	44·32	2·01	October	43·48	4·53	October		
November	48·66	2·33	November	39·10	2·28	November		
December	37·31	0·68	December	39·65	1·96	December		
Sums	541·97	25·28		549·53	26·04		270·82	8·13
Mean	45·16			45·79			45·14	

B. *General Observations on the Thermometric Curves.*

Some of the most important results depend upon the annual ranges of temperature at different depths. But the determination of the extremes is no easy matter. I will first direct attention to the curves in Plate VII., which convey a great deal of valuable information, which can here be only slightly touched upon. They are reduced to *one-sixth* of the size of the original projections, in which one degree occupied two-thirds of an inch vertically, and one day occupied one-tenth of an inch horizontally. The *corrected* temperatures are those which have been projected.

The curves extend over five years; and are placed in the order of depths (vertically) to which they belong: the uppermost undulating curves shewing the variations at the three stations 3 French feet below the surface, the lowest set shewing the variations 24 French feet below the surface.

The most obvious results are the following:—

1. In the upper set of curves, though the irregularities are greatest, yet the three curves follow one another with singular fidelity throughout these irregularities. The curves separate a little in summer, and regularly in the same direction every summer, shewing the influence of exposure, the Experimental Garden being most heated, then Craigleith, and lastly the Observatory, which is also the order of the elevations of the stations above the sea. It may also be added, that the diurnal change may possibly have some slight influence upon the Experimental Garden, where the observations were made fully two hours later than at the other stations. (See Section V.)

2. As the local irregularities diminish at increasing depths, the range diminishes, and the times of maxima and minima are continually retarded.

3. At increasing depths, the curves, which followed one another so closely and exactly amidst the irregularities of temperature near the surface, *systematically separate from one another*, both owing to a variation in the range or degree of undulation of the curve, and owing to a greater or less degree of retardation in the maxima or minima of the different curves.

4. The effect last described is least sensible in comparing the observations at the Observatory and Experimental Garden, but *most* sensible if either of these be compared with the Craigleith observations, for which last the range diminishes more slowly, so that, at 24 French feet, it is about double that of either of the others, and the retardation of the maxima and minima is much less.

5. In the trap and loose sand, the range is diminished to *one-tenth part* in descending from 3 feet to 24 feet; but in the sandstone it is not quite diminished to *a fifth part.* The epoch of maximum temperature is retarded in the two former cases nearly five months, in the latter only three.

From these statements it is easy to see that the influence of the CONDUCTING

POWER OF THE DIFFERENT SOILS OR ROCKS FOR HEAT is very palpable. But to submit it to numerical calculation, a more elaborate analysis is necessary. Each year has been first considered by itself, and then the whole united.

C. *Thermometric Ranges.*

To ascertain the range for each year, the maximum and minimum points of the curves of each thermometer were ascertained graphically by the aid of an elastic wire, bent so as to represent a curve which should pass through the zigzags of the temperature curve, and connect the observed points with tolerable accuracy. The points of greatest and least temperature in each year were thus represented with a certain degree of approximation, and the results are shewn in the following table.

TABLE VIII. SHEWING THE MAXIMUM AND MINIMUM TEMPERATURE AND RANGE FOR EACH OF FIVE YEARS.

		Observatory, Trap.			Experimental Garden, Sand.			Craigleith, Sandstone.		
		Maximum.	Minimum.	Range, Fahrenheit.	Maximum.	Minimum.	Range, Fahrenheit.	Maximum.	Minimum.	Range, Fahrenheit.
3 FEET.	1837-38	56°·25	37°·30	18°·95	57°·20	37°·55	19°·65	55°·90	37°·65	17°·25
	1838-39	53·40	35·70	17·70	[illegible]5·45	[illegible]5·12	20·33	53·90	35·38	18·52
	1839-40	53·05	38·10	15·55	56·50	37·50	19·00	54 30	37·85	16·45
	1840-41	53·87	38·95	14·92	56·35	38·10	18·25	55·10	38·95	16·15
	1841-42	52·85	38·88	13·97	54·50	37·85	17·65	53·15	38·25	14·90
6 FEET.	1837-38	52·30	40·40	11·[illegible]0	54·65	39·70	14·95	53·80	39·90	13·09
	1838-39	50·90	39·70	11·20	53·20	38·63	14·57	52·35	38·10	14·25
	1839-40	50·97	40·65	10·32	53·67	39·70	13·97	52·53	39·20	13·33
	1840-41	51·35	41·10	10·25	53 75	40·52	13 23	53·15	40 05	13·10
	1841-42	51·07	40 78	10·29	52·95	39·55	13·40	51·90	38·95	12·95
12 FEET.	1837-38	49·40	43·90	5·50	50·65	43·10	7·55	51·10	41·70	9·40
	1838-39	48·65	43·60	5·05	49·95	42·85	7·10	50·05	40·75	9·30
	1839-40	48·57	43·73	4·84	50·19	43·08	7·11	49·80	41·45	8·35
	1840-41	48·80	44·30	4·50	50·30	43·60	6·70	50·45	42·12	8·33
	1841-42	49·00	44.20	4·80	50·40	43 50	6·90	50·30	41·60	8·70
24 FEET.	1837 38	47·85	46 40	1·45	48·25	46·15	2·10	48·50	44·40	4·10
	1838-39	47·45	46 20	1·25	47·88	46·00	1·88	47·88	44·05	3·83
	1839-40	47·35	45·97	1·38	47·40	45·97	1·43	47·82	43·87	3·95
	1840-41	47·38	46·15	1·23	48·00	46·10	1·90	48·12	44·40	3·72
	1841-42	47·50	46·12	1·38	48·10	46·10	2·00	48·25	44·35	3·90

Theory shews, that the annual range ought to decrease in geometrical progression, as the depths increase uniformly. In other words, the ranges may be represented by the ordinates of a logarithmic curve. And that such is the case may be seen from the curves in Plates IX. and X., where the logarithmic curves are drawn through points so as to represent, as well as is practicable, the law of decreasing range at the different stations. These diagrams were constructed without any reference to one another; and their general coincidence is highly satisfactory.

To express the results geometrically,

$$\text{Log. } \Delta = A + B p$$

Where Δ is the thermometric range at a depth p in French feet; A and B are constants, the second of which is always negative. These constants are important, and their determination may be considered as the *primary* object of this investigation. A is manifestly equal to the logarithm of the thermometric range at the surface, or when $p=0$; B is a constant which determines the rate of diminution of the range in the interior of the earth, being smaller in proportion as the heat penetrates more readily, or as the conductivity of the soil is greater. It was shewn by FOURIER to be directly proportional to the square root of the specific heat of the soil, and inversely as the square root of the conductivity.*

These quantities A and B have reference to the thermometric scale employed, and therefore it is convenient, in order to obtain comparable results, to use the same unit as MM. POISSON and QUETELET have done in their comparison of theory with observation, that is, the centigrade scale. For this purpose, the ranges are expressed in the following table in centigrade degrees.

TABLE IX. RANGES IN CENTIGRADE DEGREES.

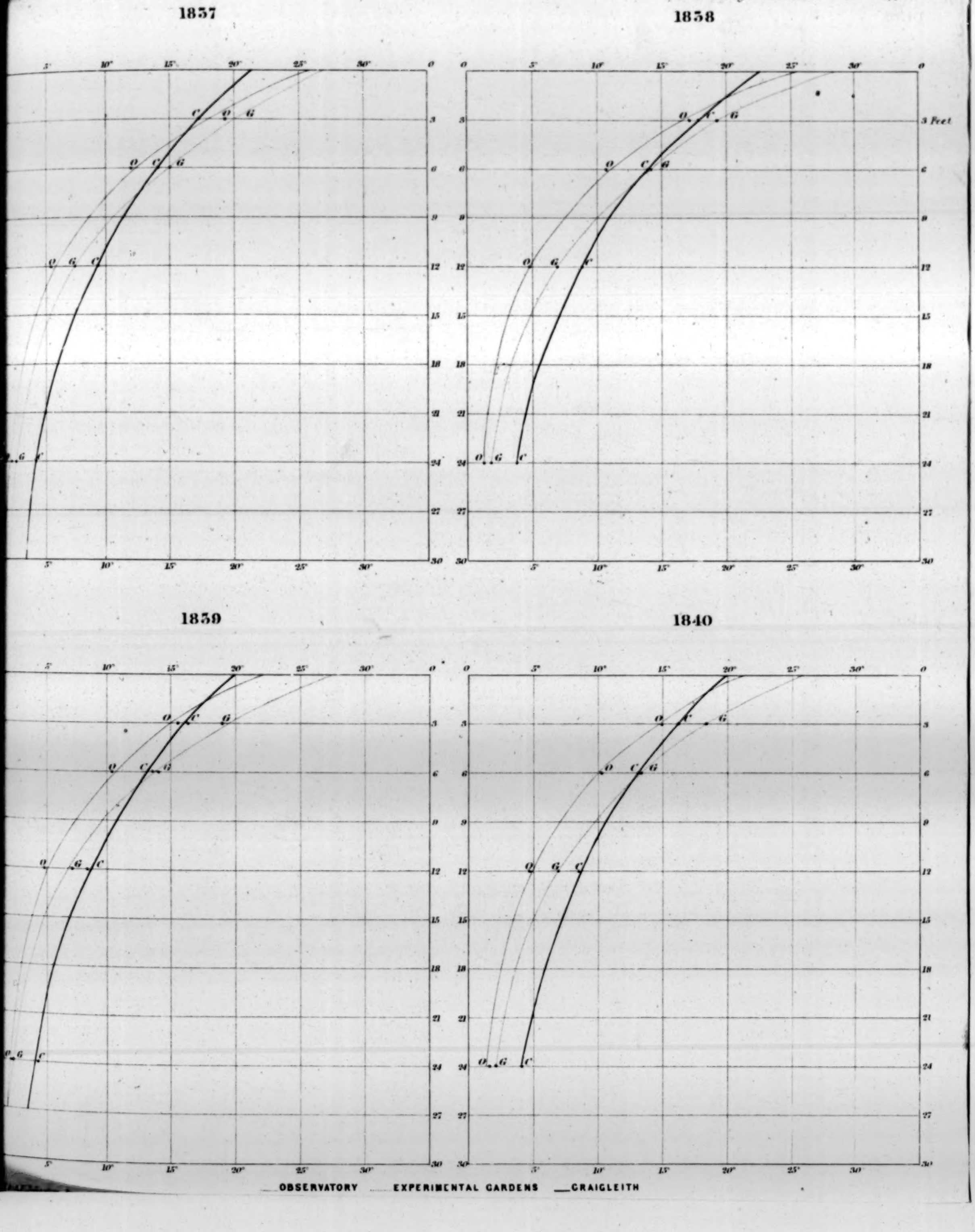
	3 Feet.			6 Feet.			12 Feet.			24 Feet.		
	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.
	°	°	°	°	°	°	°	°	°	°	°	°
1837	10·53	11·23	9·58	6·61	8·30	7·72	3·05	4·19	5·22	0·80	1·16	2·28
1838	9·83	11·30	10·29	6·22	8·10	7·91	2·80	3·94	5·16	0·70	1·05	2·13
1839	8·64	10·55	9·14	5·73	7·76	7·40	2·69	3·95	4·64	0·76	0·79	2·20
1840	8·29	10·14	8·98	5·70	7·35	7·28	2·50	3·72	4·63	0·89	1·06	2·07
1841	7·79	9·80	8·28	5·71	7·45	7·20	2·66	3·83	4·83	0·76	1·11	2·16
Means,	9·02	10·60	9·25	5·99	7·79	7·50	2·74	3·93	4·89	0·78	1·03	2·17

Two results are sufficient for eliminating the constants A and B at each station, and the most probable combination may be had by the method of least squares. I have preferred, however, the graphical method already referred to for finding, by means of a diagram and a pair of proportional compasses, the logarithmic curve which best represents the observations. This being done as shewn in Plates IX. and X., the values of A and B may be deduced thus. A, as already observed, is the logarithmic range at the surface. Taking a space equal to 10° Cent. (or 18° Fahr.) in the compasses, find the depth at which the curve has this quantity for an ordinate, let p_{10} be this depth. Then, since Log. Δ=Log. 10=1, the equation above becomes

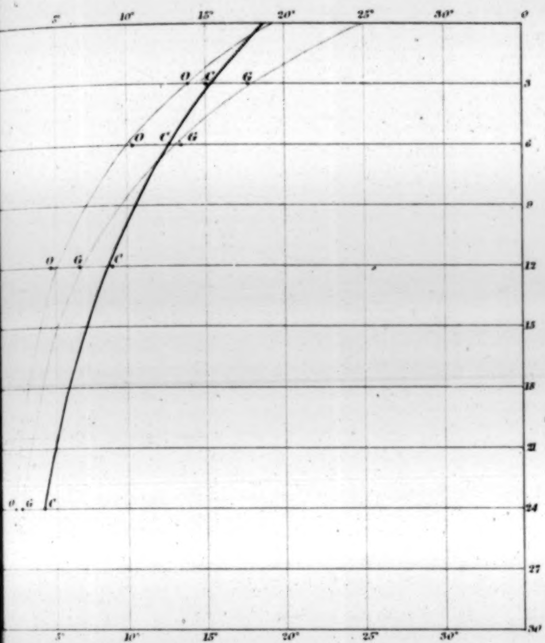
$$1 = A + B p_{10}$$

$$\text{and } B = \frac{1 - A}{p_{10}}.$$

* For farther particulars, see the Appendix at the end of this memoir, and also Sub-Section F.


1837
1838
1839
1840
O
G
C
3 Feet
OBSERVATORY
EXPERIMENTAL GARDENS
CRAIGLEITH

1841



MEAN 1837-41

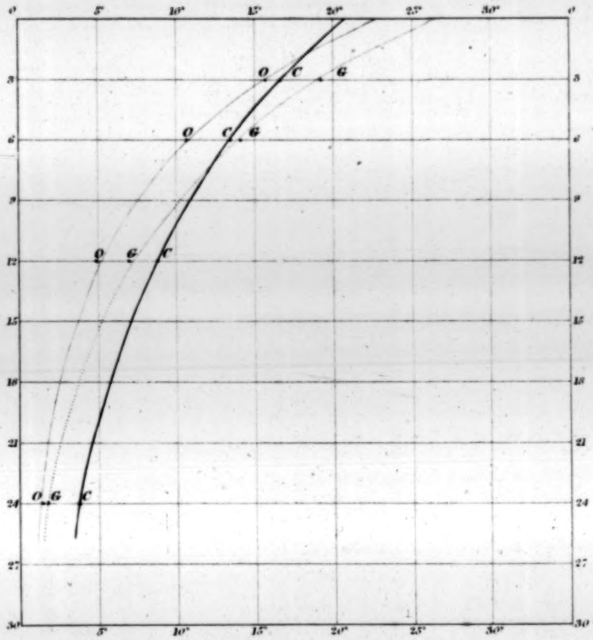
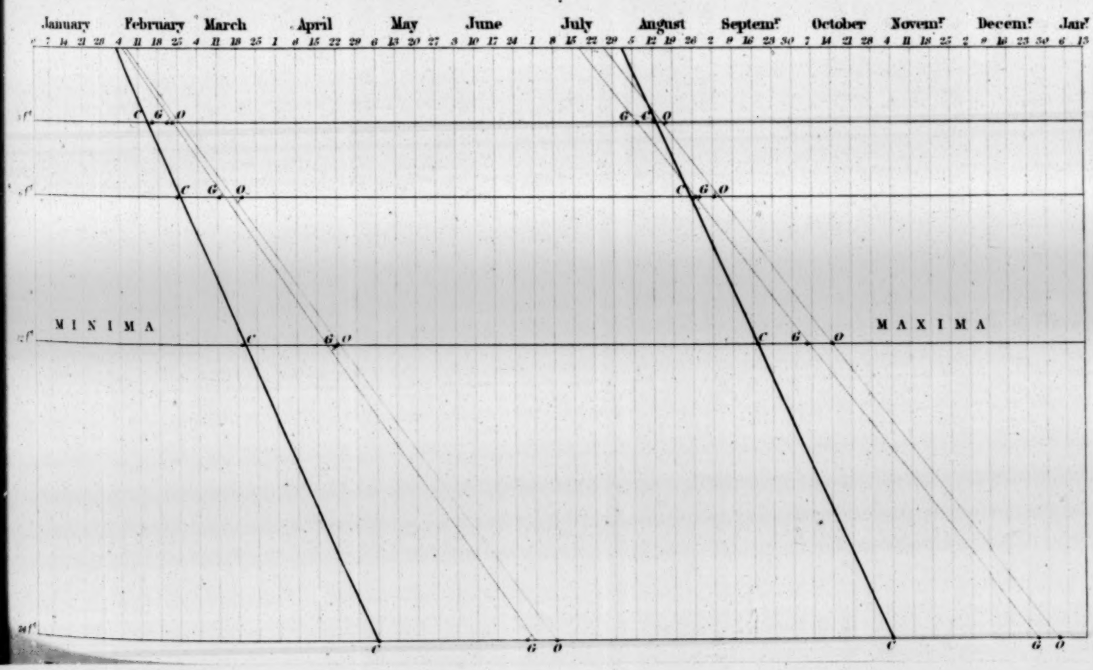


DIAGRAM SHEWING THE PROGRESS OF HEAT DOWNWARDS

BY THE MEAN OF FIVE YEARS.



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Also p_1, $p_{0\cdot1}$, $p_{0\cdot01}$ denoting the depths corresponding to a range of 1°, 0°·1, 0°·01 Cent., we have

$$p_1 = -\frac{A}{B} \qquad p_{0.1} = -\frac{1+A}{B} \qquad p_{0\cdot01} = -\frac{2+A}{B}.$$

Numerical Example. By the projection for 1837, Plate IX., we find

	Observatory.	Experimental Garden.	Craigleith.
The superficial range	14°·6 Cent.	15°·0 Cent.	11°·9 Cent.
p_{10}=depth where range=10° Cent.	3·0 F. ft.	4·0 F. ft.	2·4 F. ft.
Whence A	1·164	1·176	1·076
B	−0·0547	−0·0440	−0·0317
p_1	21·4 F. ft.	26·7 F. ft.	34·1 F. ft.
$p_{0\cdot1}$	39·7 ...	49·5 ...	65·7 ...
$p_{0\cdot01}$	58·1 ...	72·2 ...	97·3 ...

The numbers in the last line may be taken (arbitrarily) as a limit of comparison for the point at which the annual variation sensibly vanishes, and its difference in the three stations shews the marked influence of the conducting soil or rock. The following tables contain a summary of these results for five years.

TABLE X. SHEWING THE VALUES OF A AND B.*

Values of A.				Values of B.			
	Observatory.	Experimental Garden.	Craigleith.		Observatory.	Experimental Garden.	Craigleith.
1837	1·164	1·176	1·076	1837	—·0545	—·0440	—·0316
1838	1·173	1·217	1·114	1838	—·0641	—·0517	—·0345
1839	1·086	1·182	1·049	1839	—·0516	—·0498	—·0305
1840	1·073	1·155	1·044	1840	—·0550	—·0470	—·0308
1841	1·031	1·141	1·019	1841	—·0474	—·0460	—·0281
Means,	1·105	1·174	1·060	Means,	—·0545	—·0477	—·0311

TABLE XI. SHEWING THE DEPTHS AT WHICH THE ANNUAL RANGE IS REDUCED TO 0°·01 CENT.

	Observatory.	Experimental Garden.	Craigleith.
1837	58·1	72·2	97·3
1838	49·3	61·8	91·0
1839	59·2	63·5	100·0
1840	55·9	67·1	98·8
1841	63·9	68·3	107·4
Means,	57·3	66·6	98·9

* The French foot and centigrade degree are here taken as units.

On these results it may be remarked, that A, which is the logarithm of the superficial range, is necessarily variable according to the season, and that it appears, singularly enough, to have been constantly on the decrease throughout the period of these experiments. This gives a great probability that the mean of these will be very nearly an average result for this climate. The depth at which the annual variation disappears is also evidently dependent, in part, on the quality of the season. B is the only proper constant, depending solely upon the specific heat and conductivity of the soil; and the mean results of Table X. are evidently near approximations to the truth.

These computations have been made on the supposition that the logarithmic law of the diminution of the range is correct, and that the deviations from it are due to accidental errors. These deviations appear, however, to be too systematic to admit exactly of this conclusion. The observations at Craigleith coincide most nearly with theory; those at the Observatory much less so, although there is every reason to believe that the observations there were in every respect the most unexceptionable of the three. At the Observatory, the observations at great depths indicate a less rapid contraction of the range than do those at the surface, as an inspection of the curves in Plates IX. and X., and the points through which they have been drawn, sufficiently proves.

To illustrate this difference, I had the constants A and B separately computed from all the possible combinations by pairs of the observations of 1837-38, with the following results.

Table XII.

Taken in combination with	Observatory.			Experimental Garden.			Craigleith.		
	12 Feet.	6 Feet.	3 Feet.	12 Feet.	6 Feet.	3 Feet.	12 Feet.	6 Feet.	3 Feet.
24 Feet.	A = 1·08 B = —·0484	A = 1·126 B = —·0510	A = 1·182 B = —·0533	A = 1·180 B = —·0465	A = 1·204 B = —·0475	A = 1·191 B = —·0459	A = 1·077 B = —·0300	A = 1·064 B = —·0294	A = 1·070 B = —·0297
12 Feet.	. . . . .	A = 1·156 B = —·0560	A = 1·202 B = —·0598	. . . .	A = 1·216 B = —·0495	A = 1·193 B = —·0476	. . . .	A = 1·058 B = —·0283	A = 1·069 B = —·0293
6 Feet.	. . . . .	. . . . .	A = 1·225 B = —·0674	. . . . .	. . . . .	A = 1·182 B = —·0438	. . . . .	. . . . .	A = 1·075 B = —·0312
	Mean Values, .		A = 1.1594 B = —·0560	Mean Values, .		A = 1·1943 B = —·0469	Mean Values, .		A = 1·0690 B = —·0297

D. *Progress of Heat downwards.*

The curves of Plate VII. plainly shew that the periods of maximum and minimum temperature occur later and later as we descend. The epochs of maxima and minima were obtained graphically at the same time with the greatest and least temperature, in the manner already described. The results are contained in the following Table:—

TABLE XIII. EPOCHS OF MINIMUM AND MAXIMUM TEMPERATURE (BY INTERPOLATION) FROM 1837 TO 1842.

	3 Feet.			6 Feet.			12 Feet.			24 Feet.		
	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.
MINIMUM.												
1837							May 6	Apr. 30	Apr. 10	July 26	July 12	May 18
1838	Feb. 26	Mar. 3	Feb. 23	Mar. 14	Mar. 19	Mar. 3	Apl. 20	Apr. 22	M r. 20	July 18	July 8	May 12
1839	Mar. 14	Feb. 24	Feb. 24	Mar. 27	Mar. 25	Mar. 4	Apl. 30	Apr. 22	Apr. 1	July 12	June 24	May 12
1840	Mar. 1	Feb. 25	Mar. 1	Mar. 14	Mar. 15	Mar. 8	Apl. 19	Apr. 18	Mar. 21	July 5	June 26	Apr. 30
1841	Feb. 1	Feb. 1	Jan. 27	Feb. 17	Feb. 15	Feb. 2	Mar. 24	Mar. 20	Feb. 24	July 5	June 15	Apr. 5
1842	Jan. 25	Jan. 22	Jan. 25	Feb. 19	Feb. 15	Feb. 6						
Means	Feb. 18	Feb. 14	Feb. 13	Mar. 7	Mar. 7	Feb. 21	Apr. 20	Apr. 16	Mar. 21	July 13	June 29	May 3
MAXIMUM.												
1837	Aug. 6	July 31	Aug. 5	Sept. 2	Aug. 24	Aug. 19	Oct. 17	Oct. 6	Sept. 11	Jan. 8	Dec. 30	Nov. 11
1838	Aug. 8	Aug. 6	Aug. 16	Sept. 6	Aug. 31	Aug. 23	Oct. 19	Oct. 14	Sept. 19	Jan. 5	Jan. 4	Nov. 2
1839	Aug. 1	July 30	July 30	Aug. 26	Aug. 19	Aug. 14	Oct. 10	Oct. 3	Sept. 11	Jan. 8	Dec. 26	Nov. 4
1840	Aug. 23	Aug. 18	Aug. 18	Sept. 4	Sept. 2	Aug. 23	Oct. 6	Sept. 30	Sept. 9	Jan. 3	Dec. 18	Oct. 26
1841	Sept. 6	Aug. 23	Aug. 25	Sept. 24	Sept. 19	Sept. 15	Oct. 20	Oct. 16	Oct. 4	Dec. 29	Dec. 18	Nov. 3
Means	Aug. 15	Aug. 9	Aug. 13	Sept. 6	Sept. 1	Aug. 25	Oct. 14	Oct. 8	Sept. 17	Jan. 4	Dec. 25	Nov. 3

It will readily be understood, by the inspection of the curves, that these determinations are liable to considerable uncertainties,—in most cases amounting to several days. The curves at small depths are liable to many anomalous fluctuations, and even occasionally present an appearance of two minima; and at great depths the curves, though even, are so flat, that a considerable error may occur in detecting their highest and lowest points. It does not appear, however, that more real accuracy would be obtained by the methods of calculation which have usually been employed, instead of interpolating curves. We shall presently, however, shew how the two may be advantageously combined.

We thus see that the greatest cold of winter attains the depth of 24 French feet,—

At the Observatory (trap rock), on the 13th July;
At the Experimental Garden (loose sand), on the 29th June;
At Craigleith (sandstone), on the 3d May;

and that the greatest heat occurs on the 4th January, 25th December, 3d November respectively; shewing, in both cases, the very same order of facility in conducting heat which we had before deduced from the diminution of ranges, namely, that the Observatory ground is the worst conductor, that of the Experimental Garden but little better, and the rock at Craigleith by far the best.

Unfortunately, the measure of the retardation of epochs has, as yet, been so

imperfectly reduced to theory, that we cannot satisfactorily compare it with experiment;* but one law of great simplicity has long been known from theory to be approximately true, namely, that the retardation of epochs is *uniformly* greater as the depth increases. This is also easily verified graphically. By taking the depths in a vertical direction, and setting off the day of greatest or least temperature horizontally, a series of points is obtained through which a straight line should pass. I have not engraved these projections for *each* year, but that for the mean of the whole will be seen in the lower part of Plate X., where the interpolating lines in general pass so nearly through the dots that they cannot be distinguished. From these projections the mean rate of propagation downwards is easily determined, and affords a palpable illustration of the conducting powers of the soil.

TABLE XIV. SHEWING THE NUMBER OF DAYS REQUIRED BY THE IMPRESSION OF HEAT TO PASS THROUGH ONE FOOT OF SOIL.

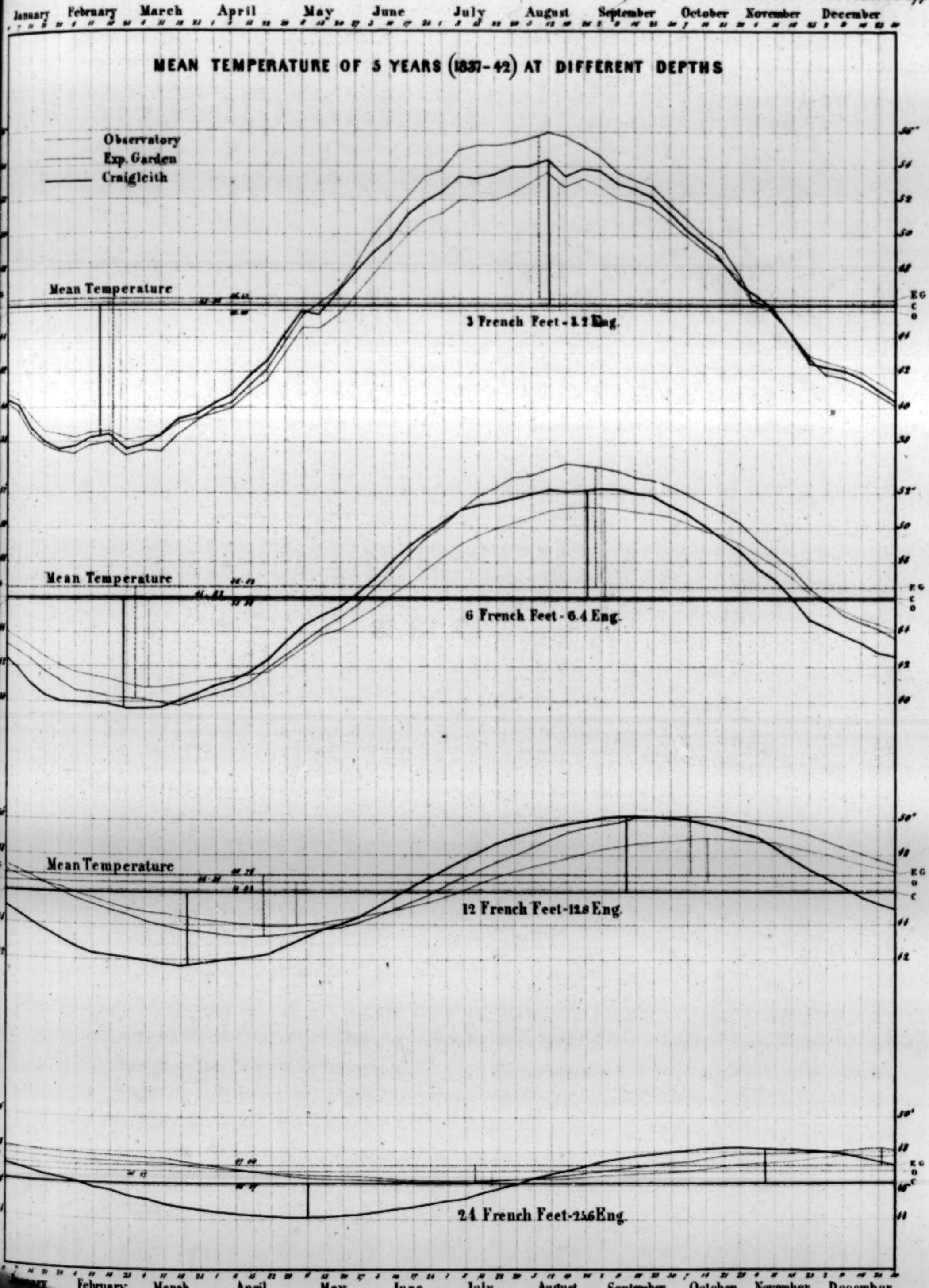
	MAXIMA.			MINIMA.		
	Observatory.	Experimental Garden.	Craigleith.	Observatory.	Experimental Garden.	Craigleith.
	Days.	Days.	Days.	Days.	Days.	Days.
1837	7·5	7·1	4·9	...	...	...
1838	6·8	6·8	3·6	6·5	5·8	3·6
1839	7·8	7·2	4·6	6·0	5·1	3·6
1840	6·6	5·95	3·5	6·1	5·7	3·05
1841	5·4	5·1	3·0	6·4	5·7	3·6
Means	6·82	6·43	3·92	6·25	5·58	3·46

It must be added, that in the several years the law of *uniform* progression is by no means accurate, although, in the mean of five, the accidents are nearly compensated. And here, again, we find the good conductor, the sandstone, gives by far the most regular and consistent results.

E.—*On the Form of the Annual Curves.*

With a view to approximate more nearly to the form of the annual curves of temperature at different depths, I have had the mean temperature for each week of the year taken by the mean of five years, which has the effect of disposing of the more irregular fluctuations, as may be seen in Plate VIII., the curves in which are taken from the following Table:

* See the Appendix.


January February March April May June July August September October November December
MEAN TEMPERATURE OF 5 YEARS (1837-42) AT DIFFERENT DEPTHS
Observatory
Exp. Garden
Craigleith
Mean Temperature
3 French Feet - 3.2 Eng.
Mean Temperature
6 French Feet - 6.4 Eng.
Mean Temperature
12 French Feet - 12.8 Eng.
24 French Feet - 25.6 Eng.
January February March April May June July August September October November December

Mean Day.	Observatory. 24 Feet.	12 Feet.	6 Feet.	3 Feet.	Experimental Garden. 24 Feet.	12 Feet.	6 Feet.	3 Feet.	Craigleith. 24 Feet.	12 Feet.	6 Feet.	3 Feet.
January 0.	47.47	46·95	44·07	40·99	47·98	47·21	43·46	40·39	47·09	44·92	42·47	40·54
„ 7.	47.47	46·73	43·68	40·96	47·95	46·86	42·99	40·16	46·93	44·46	42·09	40·47
„ 14.	47·46	46·49	43·21	39·63	47·91	46·54*	42·46	38·82	46·68	44·11	41·20	39·13
„ 21.	47·43	46·26	42·69	38·95	47·90	46·45	41·84	38·19	46·51	43·66	40·55	38·28
„ 28.	47·41	45·99	42·18	38·62	47·86	45·87	41·30	37·88	46·31	43·13	40·05	37·95
February 4.	47·54	45·77	41·82	38·35	47·86	45·52	40·85	37·45	46·23	42·95	39·97	38·13
„ 11.	47·48	45·44	41·49	38·73	47·75	45·16	41·28*	38·05	46·00	42·51	39·98	38·53
„ 18.	47·43	45·23	41·42	38·80	47·69	44·80	40·39	38·25	45·75	42·35	40·01	38·69
„ 25.	47·36	45·01	41·29	38·29	47·63	44·49	40·36	37·78	45·54	42·14	39·65	37·91
March 4.	47·30	44·79	41·04	38·54	47·50	44·29	40·14	37·94	45·32	41·99	39·65	38·22
„ 11.	47·22	44·60	40·96	38·46	47·45	44·02	39·98	37·73	45·13	41·87	39·64	38·34
„ 18.	47·15	44·42	40·99	39·32	47·33	43·82	39·98	38·82	44·95	41·59	40·08	39·50
„ 25.	47·07	44·27	41·18	39·47	47·21	43·63	40·28	39·29	44·77	41·77	40·45	39·71
April 1.	46·99	44·17	41·27	39·89	47·06	43·49	40·50	40·03	44·61	41·91	40·86	40·39
„ 8.	46·91	44·07	41·40	40·01	46·93	43·39	40·75	40·13	44·49	42·04	41·09	40·64
„ 15.	46·84	44·02	41·56	40·91	46·85	43·38	41·07	41·13	44·39	42·16	41·63	41·80
„ 22.	46·76	43·97	41·90	41·53	46·75	43·34	41·62	41·99	44·31	42·31	42·16	42·32
„ 29.	46·68	43·97	42·33	42·78	46·64	43·39	42·06	43·14	44·26	42·62	43·02	43·75
May 6.	46·61	44·02	43·03	44·54	46·51	43·51	43·08	45·38	44·23	43·10	44·18	45·43
„ 13.	46·54	44·10	43·66	44·55	46·48	43·69	43·93	45·71	44·24	43·57	44·70	45·22
„ 20.	46·47	44·23	44·01	45·02	46·39	43·92	44·51	46·35	44·30	43·96	45·21	46·34
„ 27.	46·40	44·40	44·47	46·00	46·28	44·18	45·14	47·46	44·37	44·36	45·87	47·33
June 3.	46·35	44·56	45·09	47·26	46·22	44·49	46·04	49·22	44·46	44·78	46·63	48·58
„ 10.	46·30	44·75	45·85	48·41	46·16	44·77	47·08	50·61	44·57	45·31	47·61	49·43
„ 17.	46·26	44·99	46·59	49·69	46·13	45·15	48·05	51·98	44·73	45·92	48·63	50·91
„ 24.	46·22	45·20	47·39	50·63	46·12	45·54	49·08	53·10	44·92	46·49	49·49	51·67
July 1.	46·21	45·55	48·05	51·04	46·13	46·02	49·90	53·30	45·08	46·99	49·92	52·09
„ 8.	46·19	45·87	48·66	51·74*	46·12	46·49	50·72	54·87	45·28	47·49	50·72	53·11
„ 15.	46·20	46·19	49·38	51·96*	46·12	46·95	51·55	55·04	45·51	48·06	51·11	53·09*
„ 22.	46·20	46·53	49·79	52·00*	46·23	47·04	51·99	55·12	45·74	48·50	51·22*	53·32*
„ 29.	46·23	46·85	50·09	52·32*	46·69*	48·06*	52·53*	55·19*	45·97	48·91	51·52*	53·63*
August 5.	46·25	47·13	50·37	52·96	46·29	48·28	52·78	55·45	46·20	49·25	51·79*	53·73*
„ 12.	46·30	47·42	50·67	53·57	46·37	48·67	53·10	56·06	46·44	49·48	52·12*	54·15*
„ 19.	46·35	47·70	50·99	52·75	46·46	49·05	53·45	55·61	46·66	49·78	52·02*	53·30*
„ 26.	46·40	47·93	51·06	53·10	46·51	49·33	53·42	55·25	46·88	49·96	52·07*	53·74
September 2.	46·47	48·15	51·06	52·81	46·66	49·64	53·17	54·79	47·07	50·05	52·13*	53·53
„ 9.	46·54	48·35	51·03	52·03	46·80	49·84	53·16	53·50	47·30	50·22	52·10	52·78
„ 16.	46·62	48·52	50·82	51·71	46·99	50·09	52·70	53·02	47·46	50·20	51·73	52·52
„ 23.	46·69	48·62	50·69	51·49	47·03	50·09	52·64	52·91	47·58	50·12	51·84	52·18
„ 30.	46·77	48·71	50·52	50·91	47·15	50·17	52·29	52·08	47·71	50·08	51·23	51·50
October 7.	46·86	48·77	50·28	50·03	47·26	50·20	51·93	50·92	47·83	49·97	50·73	50·38
„ 14.	46·93	48·80	49·86	49·21	47·36	50·19	51·28	50·04	47·93	49·77	49·98	49·47
„ 21.	47·01	48·79	49·45	48·62	47·46	50·11	50·80	49·40	47·97	49·50	49·42	48·81
„ 28.	47·08	48·75	49·01	47·46	47·58	50·02	50·18	48·13	48·03	49·15	48·64	47·34
November 4.	47·15	48·67	48·27	45·78	47·65	49·83	49·16	46·00	48·05	48·72	47·42	46·27
„ 11.	47·23	48·55	47·63	45·63	47·77	49·67	48·48	45·78	48·03	48·22	47·01	45·68
„ 18.	47·29	48·38	47·11	44·48	47·82	49·42	47·60	44·35	48·01	47·74	46·07	44·29
„ 25.	47·34	48·19	46·38	43·05	47·88	49·06	46·56	42·93	47·89	47·13	44·84	42·79
December 2.	47·38	47·97	45·64	42·46	47·85	48·74	45·66	41·97	47·85	46·62	44·16	42·36
„ 9.	47·42	47·71	45·18	42·38	47·97	48·38	44·95	41·81	47·63	46·08	43·81	42·19
„ 16.	47·45	47·46	44·83	42·12	47·99	48·03	44·50	41·55	47·33	45·67	43·41	41·91
„ 23.	47·47	47·19	44·41	41·70	47·98	47·59	44·22	40·94	47·29	45·18	42·89	41·18
„ 30.	47·47	46·95	44·07	40·99	47·98	47·21	43·46	40·39	47·09	44·92	42·47	40·54

The numbers marked thus * are obtained from the Mean of Four Years only.

The dates in the preceding Table are the mean of the corresponding days of observation during the five years. More correctly they ought to be about half a-day earlier; thus, the temperature of February 4 belongs to February 3·5, or to midnight of the 3d, instead of the 4th at noon, and so of the others.

The practice of denoting periodic variations of temperature by a series of the form

$$y_n = \mathrm{A} + \mathrm{B} \sin (n+b) + \mathrm{C} \sin (2n+c) + \&c.$$

(where y_n is the temperature corresponding to the fraction of the year denoted by n, and A, B, C, b, c, are constant quantities), has prevailed in Germany at least since the time of LAMBERT.* I have thought it worth while to compute the equations for each of the 12 curves, so as to facilitate comparison with the results of QUETELET† and others. But my method of proceeding has been somewhat different from his. I sketched very carefully interpolating curves through the curves of Plate VIII., so as to diminish their remaining irregularities, and having divided the horizontal space corresponding to a year into 12 equal parts (each of which may be represented by the space of 30°, the whole period of variation being 360°), I measured and inserted in a table the ordinates of the interpolated curve corresponding to these points; and with the aid of these ordinates, the equation to the curve was calculated by the aid of the tables given at the end of the second volume of DOVE's *Repertorium*. The results were as follows:—The first term is of course the mean temperature of the year, which has been taken from Table V.

TABLE XVI. CONTAINING THE EQUATIONS TO THE ANNUAL CURVES.

3 FEET.

Observatory, $y_n = 45{\cdot}49 - 7{\cdot}39 \sin (n \,.\, 30° + 43°) + 0{\cdot}362 \sin (n \,.\, 60° + 29°)$
Ex. Garden, $y_n = 46{\cdot}13 - 9{\cdot}00 \sin (n \,.\, 30° + 49°) + 0{\cdot}737 \sin (n \,.\, 60° + 63°)$
Craigleith, $y_n = 45{\cdot}88 - 8{\cdot}16 \sin (n \,.\, 30° + 47°) + 0{\cdot}284 \sin (n \,.\, 60° + 34°)$

6 FEET.

Observatory, $y_n = 45{\cdot}86 - 5{\cdot}06 \sin (n \,.\, 30° + 23°) + 0.433 \sin (n \,.\, 60° + 7°)$
Ex. Garden, $y_n = 46{\cdot}42 - 6{\cdot}66 \sin (n \,.\, 30° + 29°) + 0{\cdot}501 \sin (n \,.\, 60° + 5°)$
Craigleith, $y_n = 45{\cdot}92 - 6{\cdot}16 \sin (n \,.\, 30° + 36°) + 0.368 \sin (n \,.\, 60° + 340°)$

12 FEET.

Observatory, $y_n = 46{\cdot}36 - 2{\cdot}44 \sin (n \,.\, 30° + 344°) + 0{\cdot}075 \sin (n \,.\, 60° + 330°)$
Ex. Garden, $y_n = 46{\cdot}76 - 3{\cdot}38 \sin (n \,.\, 30° + 348°) + 0{\cdot}230 \sin (n \,.\, 60° + 319°)$
Craigleith, $y_n = 45{\cdot}92 - 4{\cdot}22 \sin (n \,.\, 30° + 13°)$

24 FEET.

Observatory, $y_n = 46{\cdot}87 - 0{\cdot}655 \sin (n \,.\, 30° + 85°)$
Ex. Garden, $y_n = 47{\cdot}09 - 0{\cdot}920 \sin (n \,.\, 30° + 275°)$
Craigleith, $y_n = 46{\cdot}07 - 1{\cdot}940 \sin (n \,.\, 30 + 327°)$

The following table contains the experimental ordinates, and those obtained from the preceding equations. The coincidence would have been somewhat closer had the mean of the 12 equidistant ordinates been taken for the mean temperature (A), instead of the mean of the entire observations.

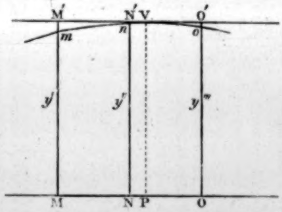
* Pyrometrie, § 675. † Ann. de l'Observatoire de Bruxelles, iv. 169.

TABLE XVII.

n.	Observatory. Obs.	Observatory. Calc.	Observatory. Diff.	Experimental Garden. Obs.	Experimental Garden. Calc.	Experimental Garden. Diff.	Craigleith. Obs.	Craigleith. Calc.	Craigleith. Diff.
3 Feet.									
0	−4·5	−4·88	−·38	−6·0	−6·10	−·10	−5·5	−5·85	−·35
1	−6·75	−6·71	+·04	−8·25	−8·21	+·04	−7·7	−7·68	+·02
2	−7·2	−7·01	+·19	−8·45	−8·57	−.12	−7·7	−7·66	+·04
3	−5·75	−5·56	+·19	−6·5	−6·60	−·10	−5·7	−5·68	+·02
4	−2·4	−2·40	·00	−2·25	−2·38	−·13	−1·85	−2·07	−·22
5	+1·25	+1·40	+·15	+3·0	+2·93	−·07	+2·3	+2·30	·00
6	+5·5	+5·22	−·28	+7·3	+7·42	+·12	+6·15	+6·16	+·01
7	+7·2	+7·43	+·23	+9·3	+9·45	+·15	+8·0	+8·25	+·25
8	+7·3	+7·39	+·09	+8·5	+8·48	−·02	+7·65	+7·90	+·25
9	+5·2	+5·22	+·02	+5·75	+5·28	−·47	+5·55	+5·36	−·19
10	+1·8	+1·78	−·02	+1·25	+1·15	−·10	+1·2	+1·50	+·30
11	−2·3	−1·87	+·43	−3·20	−2·85	+·35	−3·0	−2·55	+·45
6 Feet.									
0	−1·75	−1·92	−·17	−3·15	−3·18	−·03	−3·5	−3·75	−·25
1	−3·5	−3·64	−·14	−5·25	−5·25	·00	−5·7	−5·39	+·31
2	−4·6	−4·68	−·08	−6·2	−6·25	−·05	−5·2	−5·77	−·57
3	−4·6	−4·71	−·11	−6·0	−5·87	+·13	−5·0	−4·86	+·14
4	−3·5	−3·45	+·05	−4·0	−3·88	+·12	−2·75	−2·74	+·01
5	−0·9	−0·96	−·06	−0·65	−0·52	+·13	+0·25	+0·28	+·03
6	+2·3	+2·03	−·27	+3·3	+3·27	−·03	+4·0	+3·50	−·50
7	+4·45	+4·44	−·01	+6·2	+6·16	−·04	+5·8	+5·87	+·07
8	+5·25	+5·37	+·12	+6·8	+7·07	+·27	+6·2	+6·50	+·30
9	+4·8	+4·60	−·20	+5·75	+5·78	+·03	+5·3	+5·11	−·19
10	+3·0	+2·65	−·35	+3·3	+2·97	−·33	+2·6	+2·27	−·33
11	0·0	+0·27	+·27	−0·7	−0·30	+·40	−1·4	−1·01	+·39
12 Feet.									
0	+0·55	+0·64	+·09	+0·35	+0·55	+·20	−1·0	−0·95	+·05
1	−0·5	−0·55	−·05	−1·2	−0·97	+·23	−2·8	−2·88	−·08
2	−1·7	−1·62	+·08	−2·45	−2·28	+·17	−3·9	−4·03	−·13
3	−2·35	−2·31	+·04	−3·35	−3·15	+·20	−4·15	−4·11	+·04
4	−2·55	−2·40	+·15	−3·35	−3·28	+·07	−3·25	−3·08	+·17
5	−1·9	−1·83	+·07	−2·7	−2·49	+·21	−1·3	−1·23	+·07
6	−0·9	−0·71	+·19	−1·0	−0·85	+·15	+1·0	+0·95	−·05
7	+0·5	+0·62	+·12	+1·0	+1·11	+·11	+3·0	+2·88	−·12
8	+1·7	+1·77	+·07	+2·7	+2·74	+·04	+4·0	+4·03	+·03
9	+2·25	+2·38	+·13	+3·25	+3·45	+·20	+4·0	+4·11	+·11
10	+2·3	+2·32	+·02	+2·9	+3·14	+·24	+3·25	+3·08	−·17
11	+1·7	+1·68	−·02	+2·0	+2·03	+·03	+0·9	+1·23	+·33
24 Feet.									
0	+0·80	+0·65	−·15	+0·8	+·92	+·12	+1·0	+1·06	+·06
1	+0·6	+0·59	−·01	+0·7	+·75	+·05	+0·2	+0·10	−·10
2	+0·5	+0·38	−·12	+0·45	+·39	−·06	−0·75	−0·88	−·13
3	+0·1	+0·06	−·04	0·0	−·08	−·08	−1·5	−1·63	−·13
4	−0·25	−0·28	−·03	−0·45	−·53	−·08	−1·85	−1·94	−·09
5	−0·35	−0·54	−·19	−0·9	−·83	+·07	−1·75	−1·73	+·02
6	−0·6	−0·65	−·05	−1·05	−·92	+·13	−1·05	−1·06	−·01
7	−0·5	−0·59	−·09	−0·75	−·75	·00	0·0	−0·10	−·10
8	−0·25	−0·38	−·13	−0·3	−·39	−·09	+1·0	+0·88	−·12
9	−0·05	−0·06	−·01	+0·05	+·08	+·03	+1·75	+1·63	−·12
10	+0·30	+0·28	−·02	+0·65	+·53	−·12	+1·9	+1·94	+·04
11	+0·7	+0·54	−·16	+0·8	+·83	+·03	+1·85	+1·73	−·12

The following method of determining the absolute maxima and minima of the temperature curves and the epochs seems to be simpler in its application than those hitherto in use.

Although the temperature-curves cannot be represented, either altogether or in great part, by parabolas, the summits may always be represented sufficiently accurately by osculating parabolas, which may, of course, be determined from three points of the curve, and that with the less error as these three points approach more nearly to the point of maximum or minimum sought. In the preceding cases, the ordinates of the curve are already calculated for abscissæ corresponding to every 30°. It is easy to find, by simple inspection of the Tables, between which two ordinates the summit of the curve lies. It will necessarily be between those having the greatest values (+ or −); or, if there be two ordinates with the same value, it must be precisely half way between (supposing the portion of the curve to be parabolic).



Let y', y''', be the two greatest ordinates (calculated by the formula), and let y'' be an ordinate half way between them (calculated from the Equations, Table XVI.) Then the difference of abscissæ M N, N O, is in this case 15°. Let it be more generally m, a number always positive. Let V P be the axis of the parabola whose position is sought; and let its distance from the ordinate y'', or N P be x (+ if to the right hand, − if to the left). Then, supposing the parabola found, and the tangent to the vertex drawn, by the property of the curve,

$$a \,.\, M'm = \overline{M'V}^2$$

$$a \,.\, N'n = \overline{N'V}^2$$

$$a \,.\, O'o = \overline{O'V}^2$$

where a is the parameter. Or,

$$a\,(V\,P - y') = (m+x)^2 \quad . \quad . \quad . \quad (1)$$

$$a\,(V\,P - y'') = x^2 \quad . \quad . \quad . \quad . \quad (2)$$

$$a\,(V\,P - y''') = (m-x)^2 \quad . \quad . \quad . \quad (3)$$

Subtracting (2) from (1),

$$a\,(y'' - y') = m^2 + 2\,m\,x \quad . \quad . \quad . \quad (4)$$

Subtracting (2) from (3),

$$a\,(y'' - y''') = m^2 - 2\,m\,x \quad . \quad . \quad . \quad (5)$$

Making $y'' - y' = A$ and $y'' - y''' = B$, and adding together the last two equations.

$$a\,(A + B) = 2\,m^2$$

$$a = \frac{2\,m^2}{A+B}$$

Subtracting (5) from (4),

$$4\,m\,x = a\,(A-B)$$

and substituting the value of a just found

$$x = \frac{m}{2} \cdot \frac{A-B}{A+B}$$

which determines the position of the greatest ordinate, whence that ordinate may be deduced.

The results are contained in the following table.

TABLE XVIII.

3 FEET.	MAXIMA.				MINIMA.			
	y	x	EPOCH. Fraction of Year.	EPOCH. Month and Day.	y	x	EPOCH. Fraction of Year.	EPOCH. Month and Day.
Observatory . . .	53·20	224° 30′	·624	Aug. 16·7	36·39	50° 22′	·140	Feb. 21·0
Experimental Garden	55·73	214° 48′	·597	Aug. 7·0	37·46	50° 12′	·139	Feb. 20·5
Craigleith . . .	54·29	220° 50′	·613	Aug. 12·7	37·96	44° 42′	·124	Feb. 15·3
6 FEET.								
Observatory . . .	51·23	240° 19′	·668	Sept. 2·0	41·02	75° 47′	·211	Mar. 19·0
Experimental Garden	53·50	235° 23′	·654	Aug. 27·7	40·12	67° 30′	·188	Mar. 10·7
Craigleith . . .	52·45	234° 39′	·652	Aug. 27·0	40·13	53° 00′	·147	Feb. 23·7
12 FEET.								
Observatory . . .	48·85	282° 30′	·785	Oct. 14·7	43·86	108° 58′	·303	April 21·7
Experimental Garden	50·23	275° 41′	·766	Oct. 7·7	43·39	109° 15′	·303	April 21·7
Craigleith . . .	50·14	257° 00′	·714	Sept. 18·7	41·70	77° 00′	·214	Mar. 20·0
24 FEET.								
Observatory . . .	47·53	5° 00′	·014	Jan. 6·0	46·21	185° 00′	·514	July 7·7
Experimental Garden	48·01	355° 00′	·986	Dec. 27·0	46·17	175° 00′	·486	June 27·3
Craigleith . . .	48·01	303° 00′	·842	Nov. 4·3	44·13	123° 00′	·342	May 6·0

These results, obtained in a different manner, may be compared with those in Tables VIII. and XIII. The inspection of the deviations of the annual curve in Plate VII., from the average results in Plate VIII., illustrates well the remarkable variations in the character of the seasons in these five years, and renders it probable that the mean effects of ordinary atmospheric temperatures throughout the year may be most conveniently and accurately studied, and the annual curve ascertained, by observations at a moderate depth in the soil.

F. *On the Influence of "Specific Heat" on the Results.*

The quantity which we have, in page 208, called B (after M. QUETELET*) is equal to

$$\frac{\sqrt{\pi}}{a} \log e$$

Where $\pi = 3{\cdot}1416$, e is the base of natural logarithms, and a the symbol used by

* Annales, &c., vol. iv. p. 112.

POISSON to express the ratio $\sqrt{\frac{k}{c}}$ where k is the conductivity of the soil and c is specific heat. Whence, if the whole quantity B be known, and c the specific heat be deduced from direct experiment in the laboratory, k may be found. [In the present instance, it is to be recollected that the *French foot* is taken as the unit.]

M. ELIE DE BEAUMONT, who has taken much interest in the experiments described in this paper, very obligingly requested M. REGNAULT of Paris (whose skill in this matter is well known) to determine the specific heat of specimens taken from the grounds of the Observatory, Experimental Garden, and Craigleith respectively; and M. REGNAULT had the goodness promptly to submit them to experiment, and he communicated to me the following results:—

	Specific Heat.
Porphyry of the Calton Hill,	0·20654
Another Experiment,	0·20587
Mean,	0·20620
Sand of the Experimental Garden,	0·19432
Sandstone of Craigleith Quarry,	0·19257
Another Experiment,	0·19152
	0·19205

Some correction would, no doubt, require to be made for the moisture contained in the soil, but this appears difficult to apply, and probably would be inconsiderable. The above results evidently represent specific heats referred to unit of *weight* of the body, but that referred to in the theoretical investigation, is taken with respect to unity of volume.* The above results require, therefore, to be multiplied by the specific gravities (water being the standard in each case) which I have found to be, when reduced to 60° F.

	Trap.	Sand.	Sandstone.
Specific gravity,	2·562	1·547†	2·408
Whence we have specific heat referred to unit of *volume*,	0·5283	0·3006	0·4623

G. *Final Results.*

The value of POISSON'S constant a, expressive of the ratio $\sqrt{\frac{k}{c}}$, being obtained from our constant B by means of the relation

$$a=\frac{\sqrt{\pi}}{\mathrm{B}}\log e$$

* POISSON, Théorie de la Chaleur; *Suppl.*, p. 4.

† Mean of two experiments, 1·556 and 1·538. It is evident, that since it is required to find the specific heat of unit of volume of the mass to be heated or cooled, we must take the aggregate of sand as we find it in the soil, and not the specific gravity of the individual grains. Accordingly, the specific gravity was determined by comparing the weights of closely packed sand and of distilled water contained in a stoppered phial.

(which is equivalent to the expression in the *Theorie de la Chaleur*, p. 499, Eq. (26)), gives the following numerical result:—

Trap.	Sand.	Sandstone.
14·124	16·137	24·750

but if referred to the French METRE instead of *foot* as unity (the centigrade degree has been already employed), they become

a	4·588	5·242	8·040

which are comparable with POISSON's result, 5·11655 for the Observatory of Paris. Now, the specific heat c having been found in the last section, we may eliminate it, and obtain the following numerical values of k, the conducting power of the strata, which it may be presumed has rarely been so accurately determined for any kind of matter.

	Trap.	Sand.	Sandstone.
k	11·120	8·260	29·884

There is another constant b employed by Poisson, which involves the character of the recipient surface of the ground as well as the interior conductivity, and which is determinable from the retardation of epochs by equation (27) of page 499 of the *Theorie de la Chaleur*.

$$b=\frac{\sqrt{\pi}}{a}\left\{\cot\left[\tfrac{1}{2}(\theta+\theta_1)-\frac{x}{2a\sqrt{\pi}}\right]360^\circ-1\right\}$$

where θ and θ_1 are the epochs of maximum and minimum temperature at any given depth, reckoned from the 21st March in fractions of a year (= 1), the metre being also the unit. Instead of taking observations at a single depth, we may take the epoch for 24 French feet from the interpolating lines in Plate X., which represent not merely the observations at that depth, but the result of their combination with all the others.

	Trap.		Sand.		Sandstone.	
		Year.		Year.		Year.
Maxima at 24 F. ft.,	July 8.=	·515	July 1. =	·496	May 6.=	·234
Minima, . . .	Jan. 4.=	1·008	Dec. 26.=	·984	Nov. 4.=	·841
Mean reckoned from 1st Jan., .		·761		·740		·591
Reckoned from 21st Mar.$=\frac{1}{2}(\theta+\theta_1)$		·545		·524		·375

Substituting the values of x=7·7961 metres (24 F. ft.), and of a before found, we obtain

	Trap.	Sand.	Sandstone.
b	0·4972	0·1007	0·0772

M. POISSON finds for b at the Paris Observatory, the value 1·057. If we examine the circumstances which influence the value of b, we shall admit that its determination in this manner is liable to so great errors as to render it almost worthless.

I shall not follow farther the application of these results, of which a and k are the most immediately important. In particular, I shall not attempt to find, with POISSON, the whole climateric effect of the solar influence which he deduces from the quantities a and b found above; both on account of the uncertainty of the value of b, and because I have attempted elsewhere to shew that the physical assumptions, upon which the great French analyst has founded the determination of this quantity, are exceedingly precarious.*

I have only farther to add, that the extensive reductions and computations of which the results have been given in this paper, were performed under my immediate superintendence by different persons at different times. My thanks are due to Mr BROUN, Mr MOFFAT, Mr LINDSAY, and especially to Mr GREGG, for their attention and accuracy in conducting them.

EDINBURGH, *June* 1846.

APPENDIX,

Containing Remarks on the Connection of the Preceding Observations with the Theory of Fourier and Poisson.†

" So far as the effect of SOLAR HEAT is concerned, the *à priori* solution of the problem of the temperature of any part of the earth's surface may be thus imagined:—(1.) The *whole* quantity of sunshine which falls on any part of the earth's surface in the course of a year is to be found, and also the law of its variation of force at different seasons. (2.) The part of this heat which becomes effective in heating the earth's crust is to be found by multiplying the amount by a constant depending upon the absorbent power of the surface. (3.) This quantity of heat thus reduced is propagated towards the interior, according to the laws of conduction, which again presuppose the knowledge of two constants proper to each soil, namely, the Conductivity and the Specific Heat.

" (1.) The measure of the quantity of sunshine received by any place in a year, and its distribution at different seasons, has been a favourite problem with mathematicians. In ultimate analysis, it depends of course on the astronomical elements which affect the progress of the seasons, viz., the obliquity of the ecliptic (γ), the latitude of the place (μ), the excentricity of the earth's orbit (α), and the longitude of the sun's perigee (ϖ). But there are also elements quite as important as any of these; the imperfect transparency of the air and its varying thickness, owing to differences of obliquity of the transmitted rays, and the condition of opacity depending on the weather. Neither of these is insignificant, neither of them compensatory; both may be considered as functions of the hour-angle and fraction of the year, and the second is besides subjected to the most capricious changes. Yet of these ele-

* See Second Report on Meteorology, Arts. 104, &c., in the British Association Reports for 1840.

† Taken from the Second Report on Meteorology, British Association Report, 1840, Art. 88, &c.

ments theory has hitherto taken no account, and consequently the expression for the quantity of sunshine obtained, in terms of the astronomical constants, with so much labour, we must hold to be nearly useless as a physical datum. It is vain to say, with M. POISSON, "Les lois d'absorption de la chaleur solaire à travers l'atmosphere, les variations diurnes et annuelles sont également inconnues, et l'on peut seulement *supposer qu'elles sont peu considérables*." We know, on the contrary, that they are so considerable, that, estimating the loss of radiant heat by a *vertical* passage through the atmosphere at only twenty-five per cent., at an angle of elevation of 25° the force of the solar rays would be reduced to a half, and at 5° to *one-twentieth* part. We know, indeed, that the difference of the *direct* effect of a vertical and a horizontal sun is due to this cause alone, exaggerated, of course, immensely by the variable meteorological state of the atmosphere, which again is a function of the latitude.

"(2.) The receptive power of the surface is a datum which we find it very difficult directly to determine, and which, since the quantity of sunshine cannot (as we have seen) possibly be directly computed, must be inextricably mixed up with it. It might be a question, whether, by covering a tolerably extensive surface of soil, in which thermometers are inserted, with a composition of known superficial conductivity, this element might not become known.

"(3.) The specific heat (c) and conductivity (k) of the soil are also inextricably mixed up together in the analysis; but either becoming known, the other may be inferred from thermometric observations carried below the surface. The specific heat seems that best adapted for laboratory experiments; M. ELIE DE BEAUMONT has assigned 0·5614 for the value of c (that of an equal *bulk* of water being=1), proper to the soil at the Observatory at Paris.

"To obtain the conductivity of the soil *à posteriori*, it is fortunately not necessary that the preceding theoretical estimation of the distribution of sunshine should be correct; but there are other estimates into which it essentially enters, and which must therefore be received with corresponding caution. To facilitate reference to M. POISSON's work, I will shew how the simple and very satisfactory observation of maximum and minimum temperature of the earth's crust at given small depths (above the *invariable stratum*) may be made to yield a knowledge of some of the constants above referred to.

"Let the excess of the annual maximum above annual minimum temperature at a depth p be expressed by Δ_p; then

$$\log \Delta_p = \mathrm{A} + \mathrm{B}\, p$$

in which A of course denotes the log. range when $p = 0$ or at the surface, and B determines the common ratio of the geometrical progression according to which the range diminishes. From observations with two thermometers at different depths, A and B may be obtained *à posteriori*.

"Now when we consult M. POISSON's work, we find that his equation (23.), page 497, which is equivalent to the preceding one, is thus composed. The quantity A, on which the superficial range depends, contains (1) astronomical constants of climate $\gamma, \mu, \alpha, \varpi$ already mentioned; (2) a temperature h, depending on the mean force of the solar rays which have traversed the atmosphere and entered into combination with the earth's surface by absorption at a given place; (3) the constant of conductivity k, and of specific heat c.

"The coefficient B, on which the *rate* of diminution of the range depends, is fortunately a very simple quantity, involving neither astronomical constants, nor those proper to the superficies. It is, in fact, an absolute number multiplied by $\sqrt{\frac{c}{k}}$, and from a knowledge of it (by

observations with two or more thermometers) this quantity may be very readily and accurately determined; and it affords the only unexceptionable manner of ascertaining the conductivity of the earth's crust on a large scale.

* * * *

"The epochal retardations for the annual curves at the depth of a few feet, follow, generally speaking, a simple law, for they are propagated uniformly downwards with a velocity which is easily connected with the constants proper to the soil, determined from the range at two given depths, as just explained. It must not be concluded, however, that the epochs of earth-temperature at the surface coincide with those of air-temperature in the adjoining stratum. The difference of epoch may be obtained in terms of the conductivity and superficial characters of the solid stratum. But the complete expression for the epoch at any depth in terms of the dates of maximum and minimum at some other depth, and of the constants of conductivity and surface, derived from two observed ranges, is so complex, that, so far as I know, no attempt has been made to verify M. POISSON's formulæ except in a single example by himself, taken from M. ARAGO's observations."

TABLES.

t_1 denotes the deepest, or 24 feet Thermometer, t_2 at 12 feet, t_3 at 6 feet, t_4 at 3 feet (French Measure); t_5 at the surface of the ground; T, the temperature of the air in the Thermometer box.

OBSERVATORY.

...tes.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.	t_5.	T.
...37.	°			°	°			°	°			°	°			°		
...). 4	48·10	+·03	+·02	48·15	45·93	+·01	+·01	45·95	42·06	·00	·00	42·06	39·11	·00	−·02	39·09		
13	47·98	+·03	+·03	48·04	45·59	+·01	+·02	45·62	42·00		+·01	42·01	40·54		+·03	40·57		
20	47·87	+·03	+·02	47·92	45·37	+·01	+·01	45·39	42·17		·00	42·17	40·89		−·01	40·88		
27	47·79	+·03	+·02	47·84	45·22	+·01	·00	45·23	42·05		−·01	42·04	38·90		−·03	38·87		
...r. 6	47·73	+·03	−·01	47·75	45·09	+·01	−·02	45·08	41·68		−·03	41·65	39·39		−·07	39·92		
13	47·63	+·03	·00	47·66	44·94	+·01	−·01	44·94	41·66		−·02	41·64	39·40		−·05	39·35		
20	47·52	+·03	+·03	47·58	44·75	+·01	+·01	44·77	41·28		·00	41·28	38·08		−·01	38·07		
27	47·42	+·03	+·01	47·46	44·59	+·01	+·02	44·62	40·90		+·02	40·92	37·49		+·02	37·51		
...r. 3	47·34	+·03	+·04	47·41	44·42	+·01	+·02	44·45	40·49		+·02	40·51	37·11		+·01	37·12		
10	47·29	+·03	+·01	47·33	44·26	+·01	·00	44·27	40·32		−·01	40·31	37·69		−·04	37·65		
17	47·22	+·03	·00	47·25	44·08	+·01	−·01	44·08	40·45		−·02	40·43	38·72		−·04	38·68		
24	47·12	+·03	+·01	47·16	43·87	+·01	·00	43·88	40·76		−·01	40·75	40·09		−·03	40·06		
...y 1	47·05	+·03	−·02	47·06	43·83	·00	−·02	43·81	41·37		−·04	41·33	41·73		−·08	41·65		
8	46·96	+·02	·00	46·98	43·82	·00	·00	43·82	42·10		−·02	42·08	43·13		−·02	43·11		
15	46·92	+·02	−·04	46·90	43·90	·00	−·03	43·87	42·72		−·07	42·65	43·23		−·13	43·10		
22	46·82	+·02	−·01	46·83	44·00	·00	−·01	43·99	43·38		−·03	43·35	45·31		−·05	45·26		
29	46·78	+·01	−·05	46·74	44·16	·00	−·04	44·12	44·21		−·09	44·12	46·57		−·15	46·42		
...e 5	46·70	+·01	−·03	46·68	44·35	·00	−·03	44·32	44·94		−·07	44·87	47·42		−·10	47·32		
12	46·65	+·01	−·06	46·60	44·60	·00	−·05	44·55	45·81		−·13	45·68	48·69		−·20	48·49		
19	46·58	·00	−·07	46·51	44·88	−·01	−·04	44·83	46·80		−·14	46·66	50·99		−·10	50·89		
26	46·55	·00	−·05	46·50	45·18	−·01	−·05	45·12	47·79		−·13	47·66	52·69		−·17	52·52		
...y 3	46·53	−·01	−·07	46·45	45·55	−·01	−·06	45·48	48·85		−·15	48·70	53·46		−·19	53·27		
10	46·52	−·01	−·08	46·43	45·98	−·01	−·08	45·89	49·87		−·21	49·66						
17	46·49	−·01	−·06	46·42	46·44	−·01	−·07	46·36	50·98		−·16	50·82						
24	46·48	−·01	−·06	46·41	46·82	−·01	−·06	46·75	51·50		−·13	51·37						
31	46·49	−·02	−·03	46·44	47·32	−·01	−·04	47·27	51·79		−·06	51·73						
...g. 7	46·53	−·02	−·06	46·45	47·78	−·01	−·07	47·70	51·90		−·12	51·78	53·88		−·15	53·73		
14	46·59	−·02	−·06	46·51	48·10	−·01	−·08	48·01	51·87		−·15	51·72	54·66		−·20	54·46		
21	46·65	−·02	−·05	46·58	48·38	−·01	−·07	48·30	52·23		−·10	52·13						
28	46·70	−·02	−·03	46·65	48·60	−·01	−·04	48·55	52·49		−·05	52·44	54·40		−·04	54·36		
...pt. 4	46·78	−·02	−·03	46·73	48·87	−·01	−·04	48·82	52·11		−·05	52·06	52·67		−·05	52·62		
11	46·86	−·02	−·03	46·81	49·05	·00	−·03	49·02	51·60		−·03	51·57	52·25		−·04	52·21		
18	46·96	−·02	−·04	46·90	49·21	·00	−·05	49·16	51·41		−·07	51·34	51·48		−·10	51·38		
25	47·06	−·02	−·04	47·00	49·27	·00	−·05	49·22	51·20		−·08	51·12	52·12		−·10	52·02		
...t. 2	47·16	−·02	−·04	47·10	49·35	·00	−·07	49·28	51·09		−·11	50·98	51·10		−·15	50·95		
9	47·24	−·01	−·04	47·19	49·34	·00	−·04	49·30	50·91		−·07	50·84	51·80		−·09	51·71		
16	47·33	−·01	−·05	47·27	49·37	·00	−·05	49·32	50·88		−·08	50·80	50·84		−·11	50·73		
23	47·38	−·01	−·01	47·36	49·34	·00	·00	49·34	50·49		·00	50·49	50·28		·00	50·28		
30	47·43	−·01	+·01	47·43	49·30	·00	+·02	49·32	49·86		+·02	49·88	47·30		+·02	47·32		
...v. 6	47·50	−·01	+·02	47·51	49·25	·00	+·03	49·28	48·72		+ 05	48·77	45·05		+·03	45·08		
13	47·57	−·01	+·03	47·59	49·09	·00	+·04	49·13	47·88		+·05	47·93	45·73		+·06	45·79		
20	47·63	·00	+·03	47·66	48·86	·00	+·04	48·90	47·15		+·05	47·20	43·47		+·02	43·49		
27	47·69	·00	+·03	47·72	48·62	·00	+·04	48·66	46·42		+·04	46·46	43·35		+·03	43·38		
...c. 4	47·74	+·01	+·03	47·78	48·35	+·01	+·04	48·40	45·80		+·04	45·84	42·75		+·03	42·78		
11	47·76	+·01	+·04	47·81	48·05	+·01	+·05	48·11	45·21		+·05	45·26	41·70		+·04	41·74		
18	47·82	+·01	−·01	47·82	47·82	+·01	−·01	47·82	44·72		+·03	44·75	41·80		−·07	41·73		
25	47·84	+·01	−·01	47·84	47·45	+·01	−·01	47·45	44·41		+·03	44·44	42·79		−·05	42·74		
...838.																		
...n. 1	47·82	+·01	+·01	47·84	47·24	·00	+·01	47·25	44·80		·00	44·80	43·65		·00	43·65		
8	47·79	+·01	+·05	47·85	47·02	·00	+·05	47·07	44·70		+·06	44·76	42·50		+·07	42·57		
15	47·76	+·01	+·06	47·83	46·86	+·01	+·05	46·92	44·10		+·06	44·16	40·20		+·06	40·26		
22	47·75	+·02	+·04	47·81	46·70	+·01	+·03	46·74	43·30		+·02	43·32	38·88		·00	38·88		
29	47·71	+·02	+·05	47·78	46·41	+·01	+·04	46·46	42·41		+·03	42·44	37·80		+·02	37·82		

Dates.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.
1838.																
Feb. 5	47·69°	+·02	+·04	47·75°	46·14°	+·01	+·03	46·18°	41·79°	·00	+·02	41·81°	37·50°	·00	·00	37·50°
12	47·61	+·03	+·06	47·70	45·64	+·01	+·04	45·69	40·99		+·03	41·02	36·81		−·02	36·79
19	47·58	+·03	+·05	47·66	45·43	+·01	+·03	45·47	40·65		+·02	40·67	35·80		·00	35·80
26	47·51	+·03	+·06	47·60	45·13	+·01	+·04	45·18	40·17		+·03	40·20	35·75		+·02	35·77
Mar. 5	47·47	+·03	+·02	47·52	44·75	+·01	+·01	44·77	39·60		·00	39·60	35·70		−·02	35·68
12	47·41	+·03	+·01	47·45	44·43	+·01	·00	44·44	39·60		−·01	39·59	37·10		−·04	37·06
19	47·34	+·03	+·01	47·38	44·17	+·01	·00	44·18	40·06		−·01	40·05	38·25		−·03	38·22
26	47·25	+·03	+·01	47·29	43·97	+·01	·00	43·98	40·21		−·01	40·20	37·78		−·03	37·75
Apr. 2	47·16	+·03	+·01	47·20	43·85	+·01	·00	43·86	40·46		−·01	40·45	39·52		−·02	39·50
9	47·05	+·03	·00	47·08	43·69	·00	·00	43·69	40·75		−·02	40·73	39·91		−·04	39·87
16	46·96	+·03	+·02	47·01	43·68	·00	+·01	43·69	41·15		·00	41·15	40·89		·00	40·89
23	46·88	+·03	+·01	46·92	43·68	·00	·00	43·68	41·37		−·01	41·36	40·22		−·03	40·19
30	46·80	+·03	−·01	46·82	43·70	·00	−·01	43·69	41·59		−·03	41·56	41·00		−·07	40·93
May 7	46·75	+·02	−·02	46·75	43·73	·00	−·03	43·70	41·95		−·08	41·87	42·58		−·15	42·43
14	46·64	+·02	·00	46·66	43·75	·00	−·01	43·74	42·80		−·02	42·78	44·55		−·03	44·52
21	46·57	+·02	−·02	46·57	43·88	·00	−·02	43·86	43·37		−·04	43·33	44·02		−·09	43·93
29	46·46	+·01	−·01	46·46	44·05	·00	−·01	44·04	43·85		−·03	43·82	45·31		−·04	45·27
June 4	46·43	+·01	−·04	46·40	44·21	·00	−·03	44·18	44·44		−·07	44·37	46·09		−·14	45·95
11	46·37	+·01	−·03	46·35	44·38	·00	−·02	44·36	45·11		−·06	45·05	47·29		−·09	47·20
18	46·29	+·01	−·01	46·29	44·62	·00	−·01	44·61	45·78		−·03	45·75	48·51		−·02	48·49
25	46·26	·00	−·06	46·20	44·87	·00	−·05	44·82	46·57		−·12	46·45	49·58		−·21	49·37
July 2	46·24	·00	−·01	46·23	45·15	·00	−·02	45·13	47·36		−·03	47·33	50·82		−·01	50·81
9	46·25	−·01	−·05	46·19	45·50	−·01	−·06	45·43	48·20		−·12	48·08	52·38		−·19	52·19
16	46·25	−·01	−·05	46·19	45·83	−·01	−·06	45·76	49·10		−·12	48·98	53·26		−·17	53·09
24	46·24	−·01	−·04	46·19	46·24	−·01	−·05	46·18	49·66		−·11	49·55	52·53		−·15	52·38
30	46·26	−·01	−·05	46·20	46·57	−·01	−·06	46·50	49·84		−·12	49·72	52·39		−·18	52·21
Aug. 6	46·28	−·01	−·05	46·22	46·88	−·01	−·06	46·81	50·14		−·12	50·02	53·37		−·17	53·20
13	46·32	−·01	−·04	46·27	47·17	−·01	−·05	47·11	50·55		−·10	50·45	53·44		−·12	53·32
20	46·36	−·02	−·04	46·30	47·45	−·01	−·05	47·39	50·70		−·10	50·60	53·11		−·12	52·99
27	46·43	−·02	−·06	46·35	47·74	−·01	−·08	47·65	50·84		−·15	50·69	52·50		−·24	52·26
Sept. 3	46·47	−·02	−·04	46·41	47·93	·00	−·05	47·88	50·76		−·08	50·68	52·50		−·12	52·38
10	46·53	−·02	−·03	46·48	48·12	·00	−·04	48·08	50·70		−·05	50·65	51·04		−·08	50·96
17	46·60	−·01	−·04	46·55	48·27	·00	−·04	48·23	50·31		−·06	50·25	51·78		−·07	51·71
24	46·68	−·01	−·04	46·63	48·39	·00	−·05	48·34	50·42		−·08	50·34	51·20		−·11	51·09
Oct. 1	46·76	−·01	−·04	46·71	48·49	·00	−·05	48·44	50·30		−·09	50·21	51·29		−·12	51·17
8	46·83	−·01	−·03	46·79	48·54	·00	−·04	48·50	50·10		−·06	50·04	49·71		−·11	49·60
15	46·89	−·01	−·02	46·86	48·57	·00	−·03	48·54	49·59		−·03	49·56	48·06		−·07	47·99
22	46·98	−·01	−·03	46·94	48·58	·00	−·04	48·54	48·96		−·08	48·88	47·81		−·12	47·69
29	47·00	−·01	+·01	47·00	48·46	·00	+·02	48·48	48·70		+·03	48·73	48·09		+·05	48·14
Nov. 5	47·07	−·01	+·01	47·07	48·39	·00	+·02	48·41	48·16		+·03	48·19	45·64		+·01	45·65
12	47·13	·00	+·03	47·16	48·29	·00	+·04	48·33	47·35		+·05	47·40	44·80		+·05	44·85
19	47·19	·00	+·02	47·21	48·14	·00	+·03	48·17	46·61		+·03	46·64	43·29		+·02	43·31
27	47·22	+·01	+·05	47·28	47·86	+·01	+·06	47·93	45·57		+·08	45·65	41·85		+·08	41·93
Dec. 3	47·29	+·01	·00	47·30	47·69	+·01	·00	47·70	45·00		−·02	44·98	42·52		−·06	42·46
10	47·32	+·01	·00	47·33	47·41	·00	·00	47·41	44·91		−·01	44·90	42·55		−·04	42·51
17	47·34	+·01	+·02	47·37	47·15	·00	+·03	47·18	44·68		+·03	44·71	42·13		+·02	42·15
24	47·35	+·01	+·02	47·38	46·92	+·01	+·02	46·95	44·22		+·01	44·23	41·30		−·01	41·29
31	47·34	+·01	+·03	47·38	46·71	+·01	+·02	46·74	43·80		+·02	43·82	41·04		+·02	41·06
1839.																
Jan. 7	47·33	+·01	+·03	47·37	46·47	+·01	+·03	46·51	43·50		+·02	43·52	40·57		+·02	40·59
14	47·32	+·02	+·03	47·37	46·25	+·01	+·02	46·28	42·99		+·02	43·01	40·10		+·01	40·11
21	47·29	+·02	+·03	47·34	46·02	+·01	+·03	46·06	42·61		+·03	42·64	39·03		+·01	39·04
28	47·26	+·02	+·04	47·32	45·78	+·01	+·03	45·82	42·12		+·03	42·15	38·85		+·03	38·88
Feb. 4	47·24	+·02	+·01	47·27	45·54	+·01	+·01	45·56	41·68		−·01	41·67	37·63		−·04	37·59
11	47·22	+·02	·00	47·24	45·30	+·01	−·08	45·31	41·34		−·01	41·33	39·21		−·05	39·16
18	47·14	+·02	+·04	47·20	45·02	+·01	+·03	45·06	41·55		+·02	41·57	39·31		+·03	39·34
25	47·09	+·03	+·02	47·14	44·83	+·01	+·01	44·85	41·30		·00	41·30	38·30		−·02	38·28
Mar. 4	47·04	+·03	+·02	47·09	44·66	+·01	+·01	44·68	41·09		·00	41·09	38·97		−·02	38·95
11	46·97	+·03	+·02	47·02	44·48	+·01	+·01	44·50	41·03		·00	41·03	37·85		−·02	37·83
18	46·90	+·03	+·02	46·95	44·30	+·01	+·01	44·32	40·65		·00	40·65	38·42		−·01	38·41
25	46·84	+·03	+·01	46·88	44·12	+·01	·00	44·13	40·69		−·01	40·68	38·86		−·03	38·83
Apr. 1	46·75	+·03	+·02	46·80	43·95	+·01	+·01	43·97	40·80		·00	40·80	38·92		·00	38·92
8	46·72	+·03	−·01	46·74	43·85	+·01	−·01	43·85	40·72		−·03	40·69	38·30		−·08	38·22

…tes.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5.	T.
…839.																		
…r. 15	46·63°	+·03	·00	46·66°	43·74°	+·01	−·01	43·74°	40·68°	·00	−·02	40·66°	39·98°	·00	−·06	39·92°	41·60°	48·0°
22	46·57	+·03	−·02	46·58	43·65	·00	−·01	43·64	41·26		−·03	41·23	41·00		−·10	40·90	43·80	51·4
29	46·51	+·02	−·02	46·51	43·63	·00	−·02	43·61	41·89		−·05	41·84	42·68		−·11	42·57	47·50	53·8
…y 6	46·41	+·02	·00	46·43	43·65	·00	·00	43·65	42·67		−·01	42·66	44·50		−·01	44·49	43·60	45·6
13	46·34	+·01	·00	46·35	43·76	·00	·00	43·76	43·33		−·01	43·32	44·32		−·01	44·31	44·50	45·6
20	46·30	+·01	−·03	46·28	43·93	·00	−·02	43·91	43·60		−·07	43·53	44·18		−·13	44·05	48·60	57·6
27	46·26	+·01	−·05	46·22	44·10	·00	−·04	44·06	44·09		−·10	43·99	45·57		−·20	45·37	51·60	64·0
…ne 3	46·18	+·01	−·02	46·17	44·24	·00	−·02	44·22	44·80		−·04	44·76	47·21		−·06	47·15	48·20	51·9
10	46·15	+·01	−·04	46·12	44·45	·00	−·03	44·42	45·57		−·08	45·49	48·64		−·14	48·50	53·80	59·9
17	46·13	·00	−·06	46·07	44·71	·00	−·05	44·66	46·42		−·14	46·28	49·54		−·25	49·29	58·10	68·8
24	46·06	−·01	−·05	46·00	44·99	−·01	−·04	44·94	47·65		−·11	47·54	51·72		−·15	51·57	55·00	63·0
…y 1	46·06	−·01	−·06	45·99	45·36	−·01	−·06	45·29	48·32		−·14	48·18	51·04		−·22	50·82	53·50	67·5
8	46·06	−·01	−·07	45·98	45·72	−·01	−·07	45·64	48·76		−·16	48·60	52·69		−·26	52·43	59·20	70·0
15	46·04	−·01	−·04	45·99	46·05	−·01	−·05	45·99	49·49		−·10	49·39	53·00		−·14	52·86	54·40	61·7
22	46·05	−·01	−·05	45·99	46·42	−·01	−·06	46·35	49·87		−·11	49·76	52·81		−·15	52·66	56·20	63·2
29	46·09	−·02	−·05	46·02	46·76	−·01	−·06	46·69	50·19		−·13	50·06	53·48		−·19	53·29	56·70	65·1
…g. 5	46·14	−·02	−·06	46·06	47·01	−·01	−·08	46·92	50·62		−·16	50·46	54·12		−·22	53·90	58·00	68·3
12	46·16	−·02	−·04	46·10	47·35	−·01	−·05	47·29	50·99		−·08	50·91	53·90		−·09	53·81	54·30	60·2
19	46·19	−·02	−·01	46·16	47·62	−·01	−·01	47·60	51·02		+·01	51·03	52·63		+·05	52·68	49·70	49·8
26	46·27	−·02	−·04	46·21	47·89	−·01	−·05	47·83	50·82		−·08	50·74	52·58		−·10	52·48	54·40	59·9
…t. 2	46·33	−·02	−·04	46·27	48·07	−·01	−·06	48·00	50·81		−·09	50·72	52·50		−·12	52·38	53·40	60·9
9	46·40	−·02	−·04	46·34	48·24	·00	−·05	48·19	50·83		−·07	50·76	52·29		−·10	52·19	54·20	59·0
16	46·49	−·02	−·03	46·44	48·36	·00	−·04	48·32	50·79		−·05	50·74	51·51		−·09	51·42	52·00	57·2
23	46·58	−·02	−·04	46·52	48·50	·00	−·05	48·45	50·55		−·08	50·47	51·03		−·13	50·90	50·50	59·8
30	46·65	−·02	−·03	46·60	48·56	·00	−·04	48·52	50·28		−·06	50·22	50·61		−·08	50·53	49·20	57·0
…t. 7	46·72	−·01	−·03	46·68	48·60	·00	−·04	48·56	49·89		−·05	49·84	48·75		−·09	48·66	46·40	56·1
14	46·79	−·01	−·02	46·76	48·59	·00	−·02	48·57	49·30		−·03	49·27	49·02		−·05	48·97	48·50	53·0
21	46·88	−·01	−·03	46·84	48·55	·00	−·03	48·52	49·10		−·05	49·05	48·10		−·10	48·00	45·80	55·8
28	46·94	−·01	−·01	46·92	48·49	·00	·00	48·49	48·70		·00	48·70	48·05		·00	48·05	43·60	48·5
…v. 4	46·99	−·01	·00	47·00	48·39	·00	+·01	48·40	48·23		+·01	48·24	46·52		+·01	46·53	44·00	46·0
11	47·06	·00	−·01	47·05	48·31	·00	−·01	48·30	47·74		−·01	47·73	46·30		−·05	46·25	45·00	49·9
18	47·11	·00	·00	47·11	48·18	·00	·00	48·18	47·42		·00	47·42	46·31		−·01	46·30	43·90	47·0
25	47·14	·00	+·03	47·17	48·02	·00	+·03	48·05	47·11		+·05	47·16	45·12		+·07	45·19	39·50	39·6
…c. 2	47·17	·00	+·04	47·21	47·87	·00	+·05	47·92	46·30		+·06	46·36	42·99		+·06	43·05	34·80	36·0
9	47·20	+·01	+·04	47·25	47·68	+·01	+·05	47·74	45·44		+·06	45·50	41·60		+·05	41·65	33·50	35·9
16	47·24	+·01	+·02	47·27	47·45	+·01	+·03	47·49	44·61		+·02	44·63	41·59		+·01	41·60	38·50	40·7
23	47·28	+·01	+·01	47·30	47·18	+·01	·00	47·19	44·30		−·01	44·29	42·14		−·04	42·10	42·00	45·7
31	47·28	+·01	+·02	47·31	46·86	+·01	+·02	46·89	44·14		+·01	44·15	40·63		−·01	40·62	37·40	42·0
…840.																		
…n. 6	47·27	+·01	+·04	47·32	46·65	+·01	+·04	46·70	43·60		+·04	43·64	40·66		+·04	40·70	33·00	36·0
13	47·29	+·02	·00	47·31	46·44	+·01	·00	46·45	43·16		−·02	43·14	40·08		−·06	40·02	41·80	47·0
20	47·25	+·02	+·02	47·29	46·18	+·01	+·02	46·21	43·07		+·01	43·08	41·02		·00	41·02	36·50	40·6
27	47·22	+·02	+·04	47·28	45·91	+·01	+·03	45·95	42·90		+·04	42·94	40·49		+·05	40·54	33·00	34·9
…b. 3	47·20	+·02	+·03	47·25	45·72	+·01	+·02	45·75	42·44		+·01	42·45	39·10		·00	39·10	36·20	39·5
11	47·17	+·02	·00	17·19	45·48	+·01	·00	45·49	42·08		−·02	42·06	39·59		−·05	39·54	37·40	45·8
17	47·13	+·02	+·02	47·17	45·30	+·01	+·01	45·32	42·05		+·01	42·06	40·29		−·01	40·28	40·00	41·5
24	47·08	+·02	+·02	47·12	45·10	+·01	+·01	45·12	42·06		+·01	42·07	39·14		−·01	39·13	33·40	39·9
…r. 2	47·02	+·03	+·02	47·07	44·95	+·01	+·01	44·97	41·59		·00	41·59	38·25		−·02	38·23	32·80	41·3
9	47·01	+·03	−·03	47·01	44·81	+·01	−·02	44·80	41·24		−·05	41·19	38·40		−·13	38·27	38·90	56·0
16	46·91	+·03	+·01	46·95	44·59	+·01	·00	44·60	41·26		−·01	41·25	40·21		−·04	40·17	40·40	44·9
23	46·84	+·02	+·04	46·90	44·42	+·01	+·02	44·45	41·67		+·02	41·69	40·45		+·03	40·48	36·60	36·3
30	46·79	+·02	·00	46·81	44·34	·00	·00	44·34	41·76		−·02	41·74	40·70		−·05	40·65	42·50	46·4
…r. 6	46·72	+·02	+·01	46·75	44·26	·00	·00	44·26	42·08		−·01	42·07	41·46		−·03	41·43	41·20	44·0
13	46·69	+·02	−·04	46·67	44·26	·00	−·03	44·23	42·33		−·06	42·27	42·39		−·15	42·24	46·20	58·2
20	46·62	+·02	−·03	46·61	44·24	·00	−·03	44·21	42·90		−·06	42·84	43·63		−·13	43·50	46·30	56·3
27	46·58	+·01	−·05	46·54	44·30	·00	−·04	44·26	43·58		−·10	43·48	45·79		−·20	45·59	53·40	64·0
…y 5	46·47	+·01	+·01	46·49	44·40	·00	·00	44·40	44·73		+·01	44·74	47·71		+·05	47·76	46·20	44·0
11	46·40	+·01	+·01	46·42	44·55	·00	·00	44·55	45·31		+·01	45·32	45·72		+·02	45·74	40·00	43·5
18	46·35	+·01	−·01	46·35	44·80	·00	−·01	44·79	45·10		−·02	45·08	45·42		−·03	45·39	42·70	48·6
25	46·32	+·01	−·02	46·31	44·97	·00	−·02	44·95	45·10		−·04	45·06	46·03		−·08	45·95	48·80	53·5
…ne 1	46·31	+·01	−·05	46·27	45·10	·00	−·05	45·05	45·55		−·11	45·44	47·15		−·21	46·94	54·60	63·9
8	46·29	·00	−·07	46·22	45·24	·00	−·07	45·17	46·25		−·15	46·10	48·50		−·28	48·22	54·20	70·0
15	46·25	·00	−·04	46·21	45·40	·00	−·04	45·36	46·91		−·09	46·82	49·59		−·15	49·44	52·80	60·2

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5
1840.	°			°	°			°	°			°	°			°	°
June 22	46·24	·00	–·04	46·20	45·62	·00	–·04	45·58	47·42	·00	–·08	47·34	49·70	·00	–·13	49·57	52·50
29	46·26	–·01	–·05	46·20	45·89	·00	–·05	45·84	47·79		–·12	47·67	50·02		–·21	49·81	55·30
July 6	46·22	–·01	–·03	46·18	46·09	·00	–·03	46·06	48·25		–·06	48·19	50·60		–·07	50·53	51·70
13	46·26	–·01	–·05	46·20	46·36	–·01	–·06	46·29	48·63		–·12	48·51	50·98		–·18	50·80	52·20
20	46·26	–·01	–·04	46·21	46·59	–·01	–·04	46·54	48·99		–·08	48·91	51·43		–·13	51·30	55·70
27	46·31	–·01	–·07	46·23	46·84	–·01	–·08	46·75	49·42		–·17	49·25	51·87		–·26	51·61	57·40
Aug. 3	46·33	–·01	–·06	46·26	47·06	–·01	–·07	46·98	49·75		–·13	49·62	52·43		–·21	52·22	57·50
10	46·36	–·02	–·04	46·30	47·28	–·01	–·06	47·21	50·20		–·10	50·10	53·95		–·13	53·82	60·30
18	46·41	–·02	–·05	46·34	47·59	–·01	–·07	47·51	51·01		–·12	50·89	53·67		–·17	53·50	53·80
24	46·45	–·02	–·05	46·38	47·81	–·01	–·06	47·74	51·08		–·10	50·98	53·94		–·13	53·81	55·60
31	46·51	–·02	–·05	46·44	48·07	–·01	–·06	48·00	51·33		–·10	51·23	53·87		–·13	53·74	54·30
Sept. 7	46·57	–·02	–·04	46·51	48·30	–·01	–·06	48·23	51·40		–·09	51·31	52·90		–·12	52·78	54·80
14	46·60	–·02	–·01	46·57	48·45	·00	–·01	48·44	51·10		+·01	51·11	51·92		+·03	51·95	48·90
21	46·68	–·02	–·02	46·64	48·63	·00	–·02	48·61	50·69		–·02	50·67	50·39		–·06	50·33	49·20
28	46·76	–·02	–·05	46·69	48·74	·00	–·06	48·68	50·20		–·09	50·11	49·76		–·16	49·60	53·00
Oct. 5	46·82	–·01	–·02	46·79	48·74	·00	–·02	48·72	49·79		–·03	49·76	48·95		–·07	48·88	46·80
12	46·91	–·01	–·03	46·87	48·72	·00	–·04	48·68	49·31		–·06	49·25	48·40		–·10	48·30	46·10
19	46·96	–·01	–·02	46·93	48·64	·00	–·02	48·62	48·93		–·03	48·90	48·45		–·05	48·40	46·70
26	47·01	–·01	·00	47·00	48·54	·00	+·01	48·55	48·65		+·02	48·67	47·30		+·02	47·32	41·10
Nov. 2	47·08	–·01	·00	47·07	48·46	·00	·00	48·46	48·03		·00	48·03	46·27		–·02	46·25	45·40
9	47·12	·00	+·01	47·13	48·33	·00	+·02	48·35	47·60		+·03	47·63	45·90		+·02	45·92	42·20
17	47·18	·00	+·03	47·21	48·15	·00	+·04	48·19	46·99		+·05	47·04	44·41		+·06	44·47	39·00
23	47·25	·00	–·02	47·23	48·06	·00	–·02	48·04	46·40		–·04	46·36	43·07		–·09	42·98	43·20
30	47·29	·00	–·02	47·27	47·86	·00	–·02	47·84	45·86		–·04	45·82	43·01		–·10	42·91	46·00
Dec. 7	47·30	+·01	+·01	47·32	47·61	·00	+·01	47·62	45·50		·00	45·50	43·44		–·02	43·42	42·00
14	47·29	+·01	+·05	47·35	47·35	+·01	+·06	47·42	45·20		+·07	45·27	42·64		+·09	42·73	34·00
21	47·31	+·01	+·05	47·37	47·15	+·01	+·05	47·21	44·58		+·05	44·63	41·25		+·06	41·31	34·20
28	47·30	+·01	+·05	47·36	46·93	+·01	+·05	46·99	43·89		+·05	43·94	39·80		+·05	39·85	32·50
1841.																	
Jan. 5	47·29	+·02	+·05	47·36	46·63	+·01	+·05	46·69	43·18		+·04	43·22	39·83		+·05	39·88	31·00
11	47·28	+·02	+·05	47·35	46·38	+·01	+·05	46·44	42·72		+·04	42·76	38·31		+·04	38·35	30·20
18	47·27	+·02	+·04	47·33	46·10	+·01	+·04	46·15	41·96		+·03	41·99	37·43		+·01	37·44	30·20
25	47·25	+·03	+·04	47·32	45·79	+·01	+·03	45·83	41·39		+·02	41·41	37·25		+·01	37·26	30·80
Feb. 1	47·21	+·03	+·05	47·29	45·46	+·01	+·03	45·42	41·09		+·03	41·12	38·43		+·04	38·47	32·80
8	47·15	+·03	+·05	47·23	45·15	+·01	+·03	45·11	41·00		+·03	41·03	37·56		+·03	37·59	30·40
15	47·14	+·03	+·01	47·18	44·91	+·01	·00	44·92	40·63		–·01	40·62	37·75		–·05	37·70	39·00
22	47·10	+·03	–·01	47·12	44·66	+·01	–·01	44·66	40·87		–·03	40·84	39·46		–·08	39·38	41·30
Mar. 1	47·01	+·03	+·01	47·05	44·45	+·01	·00	44·46	41·29		–·01	41·28	39·92		–·02	39·90	36·80
8	46·97	+·03	–·02	46·98	44·34	+·01	–·02	44·33	41·40		–·04	41·36	39·89		–·11	39·78	44·00
15	46·91	+·02	–·04	46·89	44·27	·00	–·03	44·24	41·78		–·06	41·72	41·87		–·15	41·72	47·20
22	46·82	+·02	–·02	46·82	44·21	·00	–·02	44·19	42·45		–·04	42·41	42·88		–·09	42·79	44·60
29	46·75	+·02	–·03	46·74	44·24	·00	–·02	44·22	42·92		–·05	42·87	43·37		–·11	43·26	44·10
Apr. 5	46·65	+·02	·00	46·67	44·29	·00	·00	44·29	43·20		–·01	43·19	42·90		–·03	42·87	42·00
12	46·59	+·02	·00	46·61	44·37	·00	–·01	44·36	43·30		–·02	43·28	42·89		–·06	42·83	40·80
19	46·53	+·02	–·01	46·54	44·44	·00	–·01	44·43	43·38		–·04	43·34	43·10		–·09	43·01	42·80
26	46·48	+·02	–·02	46·48	44·49	·00	–·01	44·48	43·50		–·04	43·46	43·26		–·10	43·16	45·20
May 3	46·41	+·01	·00	46·42	44·53	·00	·00	44·53	43·80		–·02	43·78	44·96		–·03	44·93	42·00
10	46·39	+·01	–·03	46·37	44·61	·00	–·03	44·58	44·30		–·07	44·23	45·23		–·14	45·09	49·40
17	46·34	+·01	–·02	46·33	44·71	·00	–·02	44·69	44·80		–·04	44·76	46·55		–·09	46·46	49·00
24	46·34	+·01	–·07	46·28	44·89	·00	–·05	44·84	45·49		–·14	45·35	47·29		–·28	47·01	56·20
31	46·31	·00	–·06	46·25	45·06	·00	–·05	45·01	46·15		–·14	46·01	49·18		–·26	48·92	56·00
June 7	46·25	·00	–·02	46·23	45·24	·00	–·01	45·23	46·99		–·04	46·95	49·70		–·05	49·65	49·00
14	46·24	–·01	–·02	46·21	45·50	·00	– 02	45·48	47·50		–·05	47·45	50·39		–·06	50·33	52·20
21	46·24	–·01	–·05	46·18	45·80	–·01	–·05	45·74	48·10		–·11	47·99	50·62		–·18	50·44	54·50
28	46·21	–·01	–·03	46·17	46·07	–·01	–·03	46·03	48·45		–·06	48·39	50·60		–·11	50·49	52·60
July 5	46·22	–·01	–·03	46·18	46·35	–·01	–·03	46·31	48·80		–·06	48·76	51·90		–·09	51·81	53·60
12	46·25	–·01	–·05	46·19	46·63	–·01	–·06	46·56	49·30		–·11	49·19	51·30		–·20	51·10	54·00
19	46·27	–·01	–·04	46·22	46·87	–·01	–·04	46·82	49·44		–·09	49·35	51·79		–·13	51·66	56·00
26	46·30	–·01	–·05	46·24	47·11	–·01	–·06	47·04	49·82		–·11	49·71	52·37		–·20	52·17	58·80
Aug. 2	46·34	–·01	–·05	46·28	47·33	–·01	–·06	47·26	50·10		–·11	49·99	51·93		–·17	51·76	53·50
9	46·39	–·02	–·04	46·33	47·55	–·01	–·05	47·49	50·27		–·10	50·17	52·58		–·13	52·45	54·40
16	46·44	–·02	–·06	46·36	47·79	–·01	–·07	47·71	50·42		–·13	50·29	52·03		–·20	51·83	56·20
23	46·48	–·02	–·03	46·43	47·94	–·01	–·04	47·89	50·52		–·07	50·44	52·67		–·09	52·58	53·00

Dates.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.	t_5.	T.
841.																		
g. 30	46·54	−·02	−·03	46·49	48·10	−·01	−·05	48·04	50·70	·00	−·08	50·62	53·05	·00	−·12	52·93	57·00	60·8
pt. 6	46·59	−·02	−·02	46·54	48·26	−·01	−·03	48·22	50·90	·00	−·03	50·87	52·05		−·04	52·01	48·90	54·7
13	46·69	−·02	−·03	46·64	48·47	·00	−·04	48·43	50·71	·00	−·06	50·65	52·19		−·10	52·09	58·30	58·8
20	46·75	−·02	−·04	46·69	48·56	−·01	−·05	48·50	50·96	·00	−·08	50·88	53·25		−·12	53·13	56·80	60·7
27	46·80	−·02	−·02	46·76	48·67	−·01	−·03	48·63	51·09	·00	−·02	51·07	52·34		−·03	52·31	51·30	54·3
t. 4	46·87	−·02	−·01	46·84	48·80	·00	−·02	48·78	50·92	·00	−·01	50·91	51·34		−·03	51·31	47·80	53·5
11	46·94	−·02	−·01	46·91	48·91	·00	−·01	48·90	50·43	·00	−·01	50·42	50·07		−·03	50·04	48·60	51·9
18	47·00	−·01	·00	46·99	48·92	·00	+·01	48·93	49·90	·00	+·04	49·94	48·68		+·05	48·73	42·00	44·9
25	47·06	−·01	·00	47·05	48·89	·00	+·01	48·90	49·03	·00	+·03	49·06	46·48		+·01	46·49	41·10	44·7
ov. 1	47·13	−·01	·00	47·12	48·80	·00	·00	48·80	48·13	·00	·00	48·13	45·41		−·02	45·39	43·00	47·4
8	47·21	·00	−·01	47·20	48·65	·00	−·01	48·64	47·50	·00	−·03	47·47	45·25		−·07	45·32	47·20	52·5
15	47·23	·00	+·03	47·26	48·39	·00	+·05	48·44	47·18	·00	+·07	47·25	44·75		+·10	44·85	33·50	35·6
22	47·28	·00	+·03	47·31	48·20	+·01	+·04	48·25	46·24	·00	+·05	46·29	41·74		+·03	41·77	35·50	38·5
29	47·34	·00	·00	47·34	47·98	+·01	·00	47·99	45·20	+·01	·00	45·21	41·15		−·04	41·11	39·00	46·4
ec. 6	47·39	+·01	·00	47·40	47·68	+·01	·00	47·69	44·75	·00	·00	44·75	42·61		−·03	42·58	42·80	46·0
13	47·41	+·01	+·01	47·44	47·37	+·01	+·01	47·39	44·77	·00	·00	44·77	42·42		−·02	42·40	41·00	44·0
20	47·38	+·01	+·05	47·44	47·07	+·01	+·06	47·14	44·40	·00	+·06	44·46	40·99		+·08	41·07	31·40	31·2
27	47·41	+·01	+·02	47·44	46·86	+·01	+·02	46·89	43·62	·00	+·02	43·64	39·85		−·04	39·81	34·30	39·8
842.																		
n. 3	47·40	+·02	+·04	47·46	46·62	+·01	+·03	46·66	43·24	·00	+·03	43·27	41·06		·00	41·06	36·20	36·5
10	47·35	+·02	+·06	47·43	46·32	+·01	+·05	46·38	43·03	·00	−·05	42·98	39·42		·00	39·42	31·20	30·0
17	47·34	+·02	+·03	47·39	46·10	+·01	+·03	46·14	42·45	·00	−·02	42·43	38·42		−·03	38·39	32·40	37·2
24	47·31	+·02	+·04	47·37	45·83	+·01	+·03	45·87	41·96	·00	−·02	41·94	38·63		−·02	38·61	32·00	35·3
31	47·28	+·02	+·02	47·32	45·57	+·01	+·01	45·59	41·63	·00	·00	41·63	38·26		−·02	38·24	37·00	40·8
b. 7	47·22	+·02	+·03	47·27	45·29	+·01	+·03	45·33	41·50	·00	−·01	41·49	39·07		−·03	39·04	31·60	35·7
14	47·19	+·03	+·01	47·23	45·08	+·01	+·01	45·10	41·41	·00	·00	41·41	39·42		−·05	39·37	36·50	43·1
21	47·12	+·02	+·02	47·16	44·87	+·01	+·01	44·89	41·57	·00	·00	41·57	40·13		−·02	40·11	37·80	40·8
28	47·06	+·03	+·01	47·10	44·72	+·01	+·01	44·74	41·73	·00	·00	41·73	39·51		−·04	39·47	36·10	42·0
ar. 7	47·02	+·03	−·02	47·03	44·61	+·01	−·01	44·61	41·56	·00	−·04	41·52	39·72		−·09	39·63	41·10	53·0
14	46·95	+·03	−·01	46·97	43·49	+·01	·00	44·50	41·64	·00	−·03	41·61	40·10		−·08	40·02	41·70	51·5
21	46·85	+·02	+·01	46·88	44·37	·00	+·01	44·38	41·94	·00	·00	41·94	41·11		−·04	41·07	35·90	41·1
28	46·81	+·02	−·02	46·81	44·33	·00	−·01	44·32	42·04	·00	−·04	42·00	40·65		−·08	40·57	44·20	54·0

Experimental Garden.

Date.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.	t_5.	T.
1837.																		
eb. 4	48·13	+·05	+·03	48·21	45·41	+·02	+·01	45·44	40·99	·00	·00	40·99	38·39	·00	−·03	38·36		42
12	47·96	+·05	+·04	48·05	45·04	+·01	+·02	45·07	40·88	·00	·00	40·88	40·06		·00	40·06		40
20	47·88	+·05	+·03	47·96	44·75	+·01	+·01	44·77	41·11	·00	·00	41·11	40·53		−·01	40·52		42
27	47·85	+·05	+·03	47·93	44·61	+·01	+·01	44·63	41·21	·00	·00	41·21	38·63		−·03	38·60		42
ar. 6	47·70	+·05	+·02	47·77	44·45	+·01	·00	44·46	40·78	·00	−·01	40·77	38·99		−·04	38·95		44
13	47·63	+·05	+·01	47·69	44·25	+·01	−·01	44·25	40·72	·00	−·02	40·70	39·30		−·05	39·25		46
20	47·48	+·06	+·03	47·57	44·07	+·01	+·01	44·09	40·50	·00	·00	40·50	37·68		−·03	37·65		42
27	47·32	+·06	+·06	47·44	43·87	+·01	+·03	43·91	40·11	·00	+·02	40·13	37·58		+·02	37·60		35
pr. 3	47·20	+·06	+·06	47·32	43·68	+·01	+·03	43·72	39·79	·00	+·02	39·81	37·48		+·02	37·50		35
10	47·20	+·06	+·01	47·27	43·48	+·01	−·01	43·48	39·74	·00	−·02	39·72	38·03		−·06	37·97		46
17	47·07	+·06	−·04	47·09	43·35	+·01	−·04	43·32	39·98	·00	−·05	39·93	39·10		−·13	38·97		56
24	46·94	+·05	−·02	46·97	43·20	+·01	−·02	43·19	40·48	·00	−·04	40·44	40·63		−·08	40·55		50
ay 1	46·86	+·05	−·06	46·85	43·08	·00	−·05	43·03	41·25	·00	−·08	41·17	42·52		−·16	42·36		60
8	46·74	+·04	−·05	46·73	43·18	·00	−·04	43·14	41·98	·00	−·07	41·91	43·90		−·15	43·75		58
15	46·70	+·04	−·11	46·63	43·35	·00	−·08	43·27	42·80	·00	−·14	42·66	44·81		−·28	44·53		70
22	46·58	+·04	−·05	46·57	43·51	·00	−·04	43·47	43·60	·00	−·07	43·53	46·42		−·13	46·29		57
29	46·50	+·03	−·08	46·45	43·79	−·01	−·06	43·72	44·81	−·01	−·10	44·70	47·98		−·18	47·80		62
une 5	46·45	+·03	−·10	46·38	44·09	−·01	−·07	44·01	45·62	−·01	−·13	45·48	48·80		−·23	48·57		66

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5	T
1837.																		
June 12	46·41	+·01	−·12	46·30	44·45	−·01	−·09	44·35	46·72	−·01	−·18	46·53	50·41	·00	−·30	50·11		72
?	46·39	−·01	−·13	46·25	44·95	−·02	−·11	44·82	48·12	−·01	−·22	47·89	53·00		−·33	52·67		75
26	46·35	−·02	−·13	46·20	45·30	−·02	−·12	45 16	49·29	−·01	−·22	49·06	54·80		−·33	54·47		75
July 3	46·34	−·02	−·13	46·19	45·89	−·02	−·13	45·74	50·62	−·01	−·24	50·37	55·80		−·35	55·45		76
10	46·35	−·04	−·17	46·14	46·49	−·03	−·17	46·29	51·98	−·01	−·32	51·65	57·82		−·47	57·35		84
17	46·35	−·05	−·13	46·17	47·16	−·03	−·15	46·98	53·34	−·01	−·25	53·08	57·91		−·33	57·58		76
24	46·39	−·05	−·13	46·21	47·74	−·03	−·15	47·56	53·82	−·01	−·23	53·58	58·09		−·31	57·78		75
31	46·44	−·05	−·08	46·31	48·51	−·02	−·08	48·41	54·30	·00	−·10	54·20	56·72		−·11	56·61		63
Aug. 7	46·53	−·05	−·18	46·30	48·90	−·02	−·21	48·67	54·52	·00	−·32	54·20	56·38		−·47	55·91		82
14	46·62	−·05	−·14	46·43	49·35	−·02	−·16	49·17	54·39	·00	−·24	54·15	56·89		−·32	56·57		75
21	46·69	−·06	−·11	46·52	49·70	−·02	−·12	49·56	54·60	−·01	−·15	54·44	57·83		−·18	57·65		68
28	46·82	−·06	−·10	46·66	50·02	−·02	−·11	49·89	54·83	·00	−·14	54·69	56·34		−·19	56·15		67
Sept. 4	46·93	−·05	−·07	46·81	50·30	−·01	−·08	50·21	54·30	·00	−·09	54·21	54·40		−·13	54·27		62
11	47·03	−·05	−·05	46·93	50·46	−·01	−·04	50·41	53·54	·00	−·03	53·51	53·62		−·03	53·59		56
18	47·21	−·05	−·10	47·06	50·62	−·01	−·11	50·50	53·23	·00	−·15	53·08	52·68		−·22	52·46		67
25	47·34	−·05	−·05	47·24	50·58	−·01	−·04	50·53	52·74	·00	−·03	52·71	53·10		−·05	53·05		56
Oct. 2	47·46	−·04	−·08	47·34	50·60	·00	−·09	50·51	52·50	·00	−·11	52·39	52·00		−·16	51·84		63
9	47·60	−·04	−·05	47·51	50·55	·00	−·05	50·50	52·26	·00	−·06	52·20	52·93		−·08	52·85		58
16	47·68	−·04	−·05	47·59	50·53	·00	−·04	50·49	52·12	·00	−·05	52·07	51·61		−·08	51·53		57
23	47·75	−·04	+·02	47·73	50·42	·00	+·04	50·46	51·50	·00	+·08	51·58	50·79		+·10	50·89		44
30	47·81	−·03	+·04	47·82	50·35	·00	+·07	50·42	50·83	·00	+·10	50·93	47·98		+·10	48·08		40
Nov. 6	47·96	−·01	−·03	47·92	50·27	+·01	−·02	50·26	49·30	+·01	−·04	49·27	44·81		−·09	44·72		53
13	47·98	−·01	+·04	48·01	50·02	+·01	+·06	50·09	48·29	·00	+·06	49·35	45·95		+·06	46·01		41
20	48·04	·00	+·04	48·08	49·65	+·01	+·06	49·72	47·34	+·01	+·05	47·40	43·05		+·02	43·07		41
27	48·09	·00	+·04	48·13	49·30	+·01	+·06	49·37	46·41	+·01	+·04	46·46	43·08		+·03	43·11		40
Dec. 4	48·15	+·03	+·01	48·19	48·92	+·01	+·02	48·95	45·60	+·01	−·01	45·60	42·38		−·04	42·34		46
11	48·15	+·03	+·07	48·25	48·40	+·02	+·07	48·49	44·70	+·01	+·06	44·77	41·00		+·04	41·04		36
18	48·23	+·03	−·02	48·24	48·13	+·02	−·02	48·13	44·20	+·01	−·05	44·16	41·20		−·10	41·10		59
25	48·22	+·03	−·01	48·24	47·71	+·01	−·01	47·71	44·01	·00	−·04	43·97	41·48		−·08	41·40		50
1838.																		
Jan. 1	48·18	+·03	+·03	48·24	47·35	+·01	+·03	47·39	44·20	·00	+·01	44·21	43·34		+·01	43·35		42
8	48·10	+·03	+·09	48·22	47·06	+·01	+·08	47·15	44·06	·00	+·07	44·13	42·03		+·09	42·12		32
15	48·00	+·04	+·09	48·13	46·80	+·01	+·07	46·88	43·49	+·01	+·06	43·56	39·62		+·07	39·69		32
22	48·03	+·04	+·06	48·13	46·55	+·02	+·05	46·62	42·58	+·01	+·03	42·62	38·42		+·01	38·43		37
29	47·90	+·05	+·08	48·03	46·20	+·02	+·05	46·27	41·70	+·01	+·04	41·75	37·60		+·03	37·63		34
Feb. 5	47·89	+·05	+·07	48·01	45·80	+·02	+·05	45·87	41·00	+·01	+·03	41·04	37·10		+·01	37·11		35
12	47·85	+·05	+·02	47·92	45·58	+·02	+·01	45·61	40·52	+·01	−·01	40·52	36·50		−·04	36·46		44
19	47·70	+·07	+·07	47·84	45·05	+·02	+·04	45·11	39·90	+·01	+·01	39·92	36·00		·00	36·00		36
26	47·65	+·07	+·09	47·81	44·65	+·02	+·05	44·72	39·40	+·01	+·02	39·43	35·50		+·02	35·52		32
Mar. 5	47·65	+·07	−·07	47·65	44·45	+·02	−·06	44·41	39·02	+·01	−·07	38·96	35·38		−·14	35·24		61
12	47·55	+·08	−·03	47·60	44·01	+·02	−·03	44·00	38·62	+·01	−·04	38·59	35·06		−·10	34·96		54
19	47·42	+·07	+·01	47·50	43·59	+·02	−·01	43·60	38·60	·00	−·02	38·58	37·20		−·07	37·13		47
26	47·30	+·07	+·01	47·38	43·35	+·02	−·01	43·36	38·90	·00	−·02	38·88	37·40		−·05	37·35		45
Apr. 2	47·17	+·07	+·01	47·25	43·05	+·01	−·01	43·05	39·30	·00	−·02	39·28	39·49		−·05	39·44		45
9	47·05	+·07	−·01	47·11	42·85	+·01	−·01	42·85	39·91	·00	−·03	39·88	40·08		−·07	40·01		48
16	46·93	+·06	+·01	47·00	42·86	+·01	−·01	42·86	40·58	·00	−·02	40·56	41·05		−·04	41·01		45
23	46·80	+·05	+·01	46·86	42·91	+·01	·00	42·92	40·90	·00	−·01	40 89	40·63		−·03	40·60		44
30	46·65	+·05	+·01	46·71	42·83	·00	·00	42·83	41·30	·00	−·01	41·29	41·52		−·03	41·49	41·2	44
May 7	46·66	+·04	−·12	46·58	43·05	·00	−·08	42·97	41·92	·00	−·15	41·77	43·51		−·28	43·23	31·4?	72
14	46·50	+·04	−·01	46·53	43·12	·00	−·02	43·10	42·99	·00	−·02	42·97	45·70		−·02	45·68	42·2	48
21	46·41	+·03	−·04	46·40	43·32	·00	−·03	43·29	43·70	·00	−·06	43·64	45·23		−·11	45·12	46·9	55
28	46·30	+·03	−·05	46·28	43·62	·00	−·04	43·58	44·50	·00	−·07	44·43	46·42		−·14	46·28	50·1	58
June 4	46·30	+·01	−·08	46·23	44·00	−·01	−·07	43·92	45·32	·00	−·13	45·19	47·80		−·22	47·58	56·2	65
11	46·20	+·01	−·05	46·16	44·25	−·01	−·05	44·19	46·10	−·01	−·08	46·01	49·10		−·12	48·98	51·8	58
18	46·15	·00	−·03	46·12	44·64	−·01	−·03	44·60	46·90	−·01	−·04	46·85	50·20		−·04	50·16	52·2	53
25	46·20	−·01	−·11	46·08	45·08	−·01	−·10	44·99	48·03	−·01	−·18	47·84	51·71		−·28	51·43	58·1	71
July 2	46·12	−·01	−·04	46·07	45·50	−·02	−·03	45·45	48·98	−·01	−·04	48·93	52·81		−·02	52·79	52·1	54
9	46·15	−·03	−·07	46·05	46·00	−·02	−·07	45·91	50·04	−·01	−·11	49·92	54·51		−·13	54·38	59·1	62
16	46·16	−·04	−·09	46·03	46·43	−·02	−·09	46·32	51·08	−·01	−·16	50·91	55·50		−·20	55·30	60·4	67
23	46·26	−·04	−·11	46·11	47·00	−·02	−·11	46·87	51·63	−·01	−·18	51·44	55·00		−·25	54·75	63·0	70
30	46·30	−·04	−·11	46·15	47·50	−·02	−·12	47·36	52·10	·00	−·18	51·92	54·83		−·25	54·58	56·5	70
Aug. 6	46·30	−·05	−·09	46·16	47·90	−·02	−·10	47·78	52·50	−·01	−·14	52·35	55·90		−·17	55·73	59·1	66
13	46·42	−·05	−·09	46·28	48·37	−·02	−·11	48·24	52·91	·00	−·14	52·77	55·82		−·17	55·65	56·1	66

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5	T.
1838.																		
ug. 20	46·49	−·05	−·06	46·38	48·75	−·02	−·07	48·66	53·12	·00	−·08	53·04	55·51	·00	−·08	55·43	56·1	60
27	46·61	−·05	−·14	46·42	49·12	−·02	−·16	48·94	53·29	·00	−·24	53·05	54·82		−·33	54·49	59·2	75
pt. 3	46·66	−·05	−·09	46·52	49·35	−·01	−·11	49·23	53·11	·00	−·14	52·97	54·63		−·18	54·45	54·9	66
10	46·80	−·05	−·09	46·66	49·51	−·01	−·10	49·40	52·80	·00	−·13	52·67	52·50		−·19	52·31	53·2	65
17	46·86	−·05	−·07	46·74	49·72	−·01	−·08	49·63	52·40	·00	−·10	52·30	53·51		−·14	53·37	54·1	62
24	47·01	−·04	−·10	46·87	49·85	−·01	−·11	49·73	52·48	·00	−·16	52·32	52·71		−·22	52·49	50·6	67
t. 1	47·15	−·04	−·11	47·00	49·95	−·01	−·13	49·81	52·21	·00	−·18	52·03	52·60		−·25	52·35	53·1	69
8	47·25	−·04	−·10	47·11	49·97	·00	−·11	49·86	51·82	·00	−·16	51·66	50·70		−·23	49·47	50·2?	67
15	47·30	−·03	−·08	47·19	49·90	·00	−·09	49·81	50·99	·00	−·12	50·87	48·81		−·19	48·62	48·2	63
22	47·42	−·03	−·07	47·32	49·89	·00	−·07	49·82	50·22	·00	−·10	50·12	48·68		−·16	48·52	52·8	61
29	47·48	−·03	−·01	47·44	49·69	·00	+·01	49·70	49·82	·00	+·01	49·83	48·89		·00	48·89	42·0	49
ov. 5	47·59	−·01	+·04	47·62	49·52	·00	+·06	49·58	49·10	+·01	+·08	49·19	45·78		+·07	45·85	40·2	40
12	47·61	−·01	+·02	47·62	49·45	+·01	+·03	49·49	48·03	+·01	+·02	48·06	45·00		·00	45·00	33·4	45
19	47·67	·00	+·02	47·69	49·10	+·01	+·03	49·14	47·01	+·01	+·02	47·04	43·04		−·01	43·03	35·0	44
26	47·70	+·01	+·05	47·70	48·72	+·01	+·06	48·79	45·82	+·01	+·05	45·88	42·03		+·03	42·06	33·3	39
ec. 3	47·71	+·01	+·03	47·69	48·44	+·01	−·04	48·41	45·10	+·01	−·06	45·05	42·61		−·11	42·50	43·1	54
10	47·82	+·01	·00	47·83	48·01	+·01	·00	48·02	44·91	·00	−·02	44·89	42·05		−·06	41·99	42·1	48
17	47·81	+·03	+·03	47·87	47·61	+·01	+·03	47·65	44·51	·00	+·01	44·52	41·70		·00	41·70	36·2	42
24	47·80	+·03	+·03	47·86	47·30	+·01	+·02	47·33	44·91	+·01	+·01	44·93	40·53		−·02	40·51	38·8	43
31	47·81	+·04	+·01	47·86	46·95	+·01	+·01	46·97	43·20	·00	−·02	43·18	40·50		−·05	40·45	39·2	46
1839.																		
n. 7	47·75	+·04	+·05	47·84	46·60	+·01	+·04	46·65	42·91	·00	+·02	42·93	40·01		+·01	40·02	35·8	39
14	47·72	+·04	+·04	47·80	46·32	+·02	+·03	46·37	42·28	+·01	+·01	42·30	39·30		−·01	39·29	37·0	40
21	47·70	+·05	+·06	47·81	46·02	+·02	+·04	46·08	41·82	+·01	+·02	41·85	38·03		·00	38·03	33·0	38
28	47·70	+·05	+·07	47·82	45·61	+·02	+·05	45·68	41·20	+·01	+·03	41·24	37·90		+·02	37·92	30·4	35
eb. 4	47·53	+·05	+·06	47·64	45·30	+·02	+·04	45·36	40·60	+·01	+·02	40·63	37·01		·00	37·01	30·4	37
11	47·51	+·04	+·01	47·56	45·01	+·01	·00	45·02	44·32?	+·01	−·01	44·32	38·53		−·04	38·49	40·2	45
18	47·42	+·05	+·04	47·51	44·60	+·01	+·02	44·63	40·60	·00	·00	40·60	38·52		−·01	38·51	31·8	40
25	47·38	+·05	+·02	47·45	44·35	+·02	·00	44·37	40·30	·00	−·01	40·29	37·51		−·03	37·48	32·2	43
ar. 4	47·30	+·06	+·03	47·39	44·21	+·02	+·01	44·24	40·01	·00	−·01	40·00	38·30		−·03	38·27	36·2	42
11	47·32	+·06	+·02	47·40	43·85	+·02	·00	43·87	40·04	·00	−·01	40·03	37·40		−·04	37·36	31·9	43
18	47·05	+·06	+·01	47·12	43·70	+·01	·00	43·71	39·71	·00	−·02	39·69	38·15		−·05	38·10	35·1	45
25	47·01	+·06	·00	47·07	43·50	+·01	−·01	43·50	39·99	·00	−·02	39·97	38·80		−·05	38·75	37·0	46
pr. 1	46·85	+·06	+·03	46·94	43·32	+·01	+·01	43·34	40·05	·00	−·00	40·05	38·99		−·01	38·98	36·0	40
8	46·81	+·06	−·04	46·83	43·25	+·01	−·03	43·23	40·12	·00	−·06	40·06	38·41		−·13	38·28	38·2	55
15	46·75	+·06	−·04	46·77	43·25	+·01	−·03	43·23	40·20	·00	−·06	40·12	40·30		−·13	40·17	43·4	55
22	46·60	+·05	−·03	46·62	43·05	+·01	−·03	43·03	40·97	·00	−·05	40·92	41·51		−·10	41·41	44·8	53
29	46·51	+·05	−·04	46·52	43·20	+·01	−·03	43·18	41·01	·00	−·06	40·95	41·72		−·12	41·60	43·5?	55
ay 6	46·30	+·03	−·02	46·31	43·20	·00	−·02	43·18	42.90	·00	−·04	42·86	45·30		−·06	45·24	45·8	50
13	46·39	+·03	−·01	46·41	43·41	·00	−·01	43·40	43·70	·00	−·02	43·68	45·60		−·02	45·58	45·2	48
20	46·30	+·02	−·05	46·27	43·70	·00	−·04	43·66	44·30	·00	−·08	44·22	46·00		−·13	45·87	50·1	57
27	46·40	+·02	−·11	46·31	44·00	−·01	−·09	43·90	44·90	·00	−·15	44·75	47·30		−·28	47·02	54·0	70
une 3	46·10	+·01	−·05	46·06	44·40	−·01	−·05	44·34	45·96	−·01	−·07	45·88	49·40		−·11	49·29	50·1	57
10	46·02	·00	−·08	45·94	44·60	−·01	−·07	44·52	47·26	−·01	−·14	47·11	51·60		−·19	51·41	57·2	65
17	46·03	−·01	−·14	45·88	44·98	−·02	−·13	44·83	48·20	−·01	−·25	47·94	52·50		−·40	52·10	63·0	78
24	46·10	−·02	−·09	45·99	45·40	−·02	−·08	45·30	49·51	−·01	−·14	49·36	54·60		−·18	54·42	56·0	66
uly 1	46·20	−·03	−·13	46·04	46·00	−·02	−·13	45·85	50·50	−·01	−·24	50·25	53·60		−·33	53·27	56·1	75
8	46·20	−·03	−·13	46·04	46·50	−·02	−·14	46·34	51·20	−·01	−·24	50·95	56·10		−·33	55·77	61·0	75
15	46·20	−·04	−·10	46·06	47·01	−·02	−·11	46·88	52·10	−·01	−·17	51·92	56·10		−·21	55·89	58·8	68
22	46·30	−·04	−·09	46·17	47·50	−·02	−·09	47·39	52·60	−·01	−·13	52·46	55·61		−·16	55·45	59·9	65
29																		
ug. 5	46·40	−·04	−·13	46·23	48·49	−·02	−·15	48·32	53·52	−·01	−·22	53·29	57·10		−·30	56·80	60·4	74
12	46·41	−·06	−·09	46·26	48·85	−·02	−·10	48·73	53·73	·00	−·12	53·61	56·70		−·14	56·56	57·3	65
19	46·49	−·06	−·02	46·41	49·28	−·02	−·01	49·25	53·80	·00	+·03	53·83	55·00		+·07	55·07	51·0	51
25	46·55	−·06	−·08	46·41	49·53	−·01	−·09	49·43	53·52	·00	−·11	53·41	54·81		−·15	54·66	56·2	64
ept. 2	46·73	−·06	−·07	46·60	49·90	−·01	−·08	49·81	53·30	·00	−·10	53·20	54·30		−·13	54·17	52·2	62
9	46·85	−·05	−·10	46·70	49·96	−·01	−·12	49·83	53·30	·00	−·16	53·14	54·38		−·23	54·15	57·4	68
16	46·92	−·05	−·06	46·81	50·05	−·01	−·07	49·97	53·02	·00	−·08	52·94	53·20		−·11	53·09	54·4	60
23	47·08	−·05	−·10	46·93	50·18	−·01	−·11	50·06	52·70	·00	−·15	52·55	52·60		−·21	52·39	52·8	67
30	47·17	−·04	−·09	47·04	50·22	−·01	−·10	50·11	52·28	·00	−·14	52·14	52·12		−·20	51·92	50·0	65
ct. 7	47·26	−·04	−·06	47·16	50·19	·00	−·06	50·13	51·68	·00	−·07	51·61	49·82		−·12	49·70	47·4	59
14	47·30	−·04	−·04	47·22	50·20	·00	−·03	50·17	50·84	·00	−·04	50·80	50·01		−·07	49·94	49·0	55
21	47·42	−·03	−·07	47·32	50·01	·00	−·07	49·94	50·61	·00	−·09	50·52	48·82		−·14	48·68	43·0	60

Dates.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.	t_5
1839.																	
Oct. 28	47°·52	−·03	−·04	47°·45	49°·91	·00	−·04	49°·87	50°·01	·00	−·06	49°·95	49°·02	·00	−·09	48°·93	44°·0
Nov. 3	47·50	−·02	+·02	47·50	49·60	·00	+·04	49·64	49·20	·00	+·04	49·26	47·10		+·04	47·14	43·0
11	47·70	−·01	+·02	47·71	49·40	+·01	+·03	49·44	48·44	·00	+·02	48·46	46·60		+·02	46·62	41·0
18	47·70	−·01	+·02	47·71	49·40	+·01	+·03	49·44	48·20	·00	+·02	48·22	46·70		+·02	46·72	42·0
25	47·72	·00	+·04	47·76	49·06	+·01	+·05	49·12	47·70	·00	+·05	47·75	45·03		+·04	45·07	39·2
Dec. 2	47·75	·00	+·07	47·82	48·74	+·01	+·08	48·83	46·60	+·01	+·09	46·70	42·70		+·08	42·78	33·4
9	47·73	+·01	+·09	47·83	48·50	+·02	+·09	48·61	45·50	+·01	+·09	45·60	41·01		+·07	41·08	31·5
16	47·80	+·02	+·04	47·86	48·30	+·02	+·05	48·37	44·50	+·01	+·03	44·54	41·03		+·01	41·04	37·0
23	47·75	+·02	+·01	47·78	47·70	+·02	+·01	47·73	44·03	·00	−·01	44·02	41·90		−·04	41·86	41·8
31	47·80	+·03	+·04	47·87	47·20	+·02	+·04	47·26	43·60	+·01	+·02	43·63	40·10		·00	40·10	34·0
1840.																	
Jan. 6	47·80	+·03	+·04	47·87	47·02	+·02	+·04	47·08	43·20	+·01	+·02	43·23	40·02		·00	40·02	31·8
13	47·73	+·04	+·02	47·79	46·61	+·02	+·01	46·64	42·53	+·01	−·01	42·53	39·10		−·05	39·05	41·5
20	47·70	+·04	+·06	47·80	46·30	+·02	+·04	46·36	42·20	·00	+·02	42·22	40·30		+·03	40·33	36·0
27	47·68	+·04	+·07	47·79	45·92	+·02	+·05	45·99	42·10	·00	+·03	42·13	39·80		+·04	39·84	34·0
Feb. 3	47·60	+·05	+·06	47·71	45·62	+·02	+·04	45·68	41·61	+·01	+·03	41·65	38·02		+·01	38·03	35·2
10	47·52	+·05	−·01	47·56	45·30	+·02	−·02	45·30	41·10	·00	−·04	41·06	39·00		−·08	38·92	36·0
17	47·50	+·05	+·03	47·58	45·00	+·02	+·01	45·03	41·02	·00	·00	41·02	39·70		−·03	39·67	40·4
24	47·40	+·05	+·02	47·47	44·61	+·01	·00	44·62	41·30	·00	−·01	41·29	38·72		− 04	38·68	33·1
Mar. 2	47·30	+·06	+·02	47·38	44·50	+·02	·00	44·52	40·80	+·01	−·01	40·80	37·60		−·03	37·57	32·0
9	47·22	+·06	−·03	47·25	44·30	+·02	−·03	44·29	40·30	·00	−·05	40·25	37·60		−·11	37·49	37·0
16	47·20	+·06	·00	47·26	44·10	+·01	−·01	44·10	40·20	·00	−·03	40·17	39·50		−·07	39·43	42·0
23	47·10	+·05	+·02	47·17	43·80	+·01	·00	43·81	40·80	·00	−·01	40·79	40·10		−·03	40·07	
30	47·01	+·05	−·03	47·03	43·75	+·01	−·03	43·73	41·10	·00	−·05	41·05	41·05		−·11	40·94	
Apr. 6	46·75	+·05	·00	46·80	43·65	+·01	−·01	43·65	41·44	·00	−·03	41·41	41·50		−·06	41·44	
13	46·70	+·04	−·08	46·68	43·70	·00	−·06	43·64	41·90	·00	−·10	41·80	42·70		−·21	42·49	
20	46·70	+·04	−·07	46·67	43·65	·00	−·05	43·60	42·70	·00	−·09	42·61	44·10		−·18	43·92	
27	46·70	+·03	−·13	46·60	43·85	·00	−·09	43·76	43·60	−·01	−·16	43·43	47·01		−·32	46·69	55·2
May 4	46·50	+·02	−·07	46·45	44·01	−·01	−·06	43·94	45·02	−·01	−·11	44·90	49·01		−·16	48·85	53·2
11	46·42	+·02	+·01	46·45	44·30	−·01	·00	44·27	45·80	·00	+·02	45·82	46·60		+·05	46·65	41·8
18	46·40	+·01	−·04	46·37	44·68	·00	−·04	44·64	45·80	·00	−·06	45·74	46·52		−·10	46·42	45·5
25	46·30	+·01	−·08	46·23	45·01	·00	−·07	44·94	45·90	·00	−·11	45·79	47·70		−·19	47·51	51·8
June 1	46·30	·00	−·07	46·23	45·23	−·01	−·07	45·15	46·72	·00	−·11	46·61	49·60		−·16	49·44	55·5
8	46·34	−·01	−·16	46·17	45·45	−·01	−·15	45·29	47·60	·00	−·25	47·35	50·95		−·42	50·53	60·2
15	46·30	−·01	−·09	46·20	45·75	−·01	−·08	45·66	48·50	·00	−·13	48·37	51·80		−·19	51·61	55·2
22	46·30	−·02	−·09	46·19	46·14	−·01	−·08	46·05	49·20	·00	−·14	49·06	52·12		−·20	51·92	57·8
29	46·32	−·02	−·13	46·17	46·45	−·01	−·14	46·30	49·60	·00	−·22	49·38	52·43		−·35	52·08	60·2
July 6	46·30	−·03	−·08	46·19	46·82	−·01	−·08	46·73	50·20	·00	−·12	50·08	52·80		−·15	52·65	56·0
13	46·28	−·03	−·12	46·13	47·20	−·01	−·13	47·06	50·70	·00	−·21	50·49	53·48		−·32	53·16	55·8
20	46·51	−·03	−·11	46·37	47·63	−·02	−·12	47·49	51·18	·00	−·20	50·98	53·92		−·27	53·65	59·2
27	46·50	−·04	−·11	46·35	47·98	−·02	−·12	47·84	51·63	·00	−·18	51·45	54·50		−·26	54·24	60·8
Aug. 3	46·51	−·05	−·12	46·34	48·32	−·02	−·14	48·16	52·10	·00	−·22	51·88	55·20		−·30	54·90	62·0
10	46·51	−·05	−·07	46·39	48·50	−·02	−·09	48·39	52·70	·00	−·11	52·59	56·80		−·11	56·69	61·8
17	46·60	−·05	−·13	46·42	48·90	−·02	−·15	48·73	53·60	−·01	−·22	53·37	56·30		−·31	55·99	55·8
24	46·55	−·06	−·09	46·40	49·25	−·02	−·10	49·13	53·62	·00	−·12	53·50	56·30		−·16	56·14	58·5
31	46·80	−·06	−·13	46·61	49·60	−·02	−·15	49·43	53·90	·00	−·21	53·69	56·20		−·29	55·91	55·2
Sept. 7	46·82	−·06	−·09	46·67	49·85	−·01	−·11	49·73	53·80	·00	−·13	53·67	54·70		−·18	54·52	56·2
14	47·00	−·06	−·05	46·89	50·50	−·01	−·06	50·43	53·40	·00	−·06	53·34	53·49		−·08	53·41	49·4
21	47·13	−·05	−·07	47·01	50·25	−·01	−·07	50·17	52·81	·00	−·09	52·72	51·97		−·13	51·84	51·0
28	47·24	−·04	−·07	47·13	50·31	·00	−·07	50·24	52·12	·00	−·09	52·03	51·11		−·14	50·97	54·2
Oct. 5	47·35	−·04	−·06	47·25	50·28	·00	−·05	50·23	51·53	·00	−·06	51·47	50·10		−·11	49·99	48·4
12	47·50	−·03	−·07	47·40	50·20	·00	−·07	50·13	50·70	·00	−·09	50·61	49·11		−·15	48·96	48·5
19	47·52	−·03	−·03	47·46	50·06	·00	−·02	50·04	50·29	·00	−·03	50·26	49·50		−·05	49·45	48·0
26	47·60	−·02	+·01	47·59	49·80	·00	+·03	49·83	49·80	·00	+·04	49·84	47·90		+·03	47·93	38·2
Nov. 2	47·56	−·02	−·04	47·50	49·57	·00	−·03	49·54	49·10	·00	−·05	49·05	46·60		−·10	46·50	46·2
9	47·70	−·01	+·03	47·72	49·50	+·01	+·04	49·55	48·40	·00	+·04	48·44	46·01		+·04	46·05	41·2
16	47·80	·00	+·01	47·81	49·35	+·01	+·02	49·38	47·60	+·01	+·02	47·63	44·01		−·02	43·99	38·2
23	47·80	+·01	+·02	47·83	48·90	+·01	+·02	48·93	46·10	·00	+·01	46·11	43·00		−·02	42·98	41·2
30	47·80	+·01	−·04	47·77	48·75	+·01	−·04	48·72	45·90	+·01	−·06	45·85	42·40		−·13	42·27	45·8
Dec. 7	47·90	+·01	+·02	47·93	48·30	+·01	+·02	48·33	45·20	·00	+·01	45·21	42·80		−·01	42·79	41·2
14	47·90	+·02	+·07	47·99	47·91	+·01	+·07	47·99	44·80	·00	+·06	44·86	41·90		+·06	41·96	32·2
21	47·89	+·03	+·07	47·99	47·60	+·02	+·07	47·69	44·10	+·01	+·05	44·16	40·30		+·04	40·34	33·0
28	47·80	+·03	+·09	47·92	47·20	+·02	+·08	47·30	43·20	+·01	+·06	43·27	38·60		+·05	38·65	30·2

ates.	t_1 uncor-rected.	Cor. for Col.	Cor. for Air.	t_1 cor-rected.	t_2 uncor-rected.	Cor. for Col.	Cor. for Air.	t_2 cor-rected.	t_3 uncor-rected.	Cor. for Col.	Cor. for Air.	t_3 cor-rected.	t_4 uncor-rected.	Cor. for Col.	Cor. for Air.	t_4 cor-rected.	t_5	T.
841.																		
n. 4	47°·70	+·04	+·07	47°·81	46°·80	+·02	+·05	46°·87	42°·30	+·01	+·03	42°·34	38°·60	·00	+·02	38°·62	32°·20	36°
11	47·75	+·05	+·09	47·89	46·40	+·02	+·07	46·49	41·70	+·01	+·05	41·76	37·50		+·03	37·53	29·20	32
18	47·75	+·05	+·07	47·87		+·02	+·05		40·95	+·01	+·02	40·98	36·50		·00	36·50	30·50	36
25	47·70	+·06	+·07	47·83	45·60	+·02	+·05	45·67	40·30	+·01	+·02	40·33	36·50		+·01	36·51	30·20	35
b. 1	47·60	+·06	+·08	47·74	45·20	+·02	+·04	45·26	39·90	·00	+·02	39·92	36·70		+·02	36·72	31·50	34
8	47·50	+·06	+·08	47·64	44·75	+·02	+·05	44·82	39·60	·00	+·02	39·62	36·30		+·02	36·32	30·80	33
15	47·50	+·07	+·02	47·59	44·45	+·02	·00	44·47	39·30	·00	−·02	39·28	36·60		−·05	36·55	38·80	45
22	47·45	+·06	−·02	47·49	44·10	+·02	−·02	44·10	39·60	·00	−·04	39·56	38·70		−·09	38·61	41·20	51
ar. 1	47·25	+·06	+·02	47·33	43·80	+·01	·00	43·81	40·20	·00	−·01	40·19	39·10		−·03	39·07	39·20	43
8	47·30	+·06	−·05	47·31	43·72	+·01	−·04	43·69	40·40	·00	−·06	40·34	39·70		−·13	39·57	44·00	56
15	47·20	+·05	−·03	47·22	43·60	+·01	−·03	43·58	41·01	·00	−·05	40·96	41·90		−·10	41·80	45·20	53
22	47·00	+·05	−·04	47·01	43·60	·00	−·04	43·56	41·70	·00	−·06	41·64	42·80		−·12	42·68	45·80	55
29	46·80	+·04	−·07	46·77	43·65	·00	−·05	43·60	42·40	·00	−·09	42·31	43·50		−·19	43·31	44·20	61
pr. 5	46·70	+·04	·00	46·64	43·75	·00	−·01	43·74	42·70	·00	−·03	42·67	43·00		−·05	42·95	42·20	48
12	46·72	+·04	−·06	46·70	43·90	·00	−·05	43·85	43·05	·00	−·09	42·96	43·20		−·17	43·03	46·25	60
19	46·62	+·03	−·04	46·61	44·00	·00	−·04	43·96	43·30	·00	−·07	43·23	43·58		−·12	43·46	44·25	56
26	46·50	+·03	−·03	46·50	44·20	·00	−·03	44·17	43·50	·00	−·05	43·45	43·62		−·08	43·54	46·50	52
ay 3	46·48	+·02	−·02	46·48	44·35	·00	−·02	44·33	44·00	·00	−·04	43·96	45·90		−·06	45·84	42·75	51
10	46·43	+·02	−·07	46·38	44·48	·00	−·06	44·42	44·60	·00	−·10	44·50	46·30		−·19	46·11	52·25	62
17	46·40	+·01	−·06	46·35	44·61	−·01	−·05	44·55	45·50	·00	−·10	45·40	48·20		−·15	48·05	52·00	60
24	46·30	·00	−·15	46·15	44·90	−·01	−·14	44·75	46·30	·00	−·25	46·05	49·10		−·42	48·68	62·25	80
31	46·35	·00	−·14	46·21	45·20	−·01	−·13	45·06	47·30	−·01	−·24	47·05	51·60		−·38	51·22	60·25	78
ne 7	46·30	−·01	−·06	46·23	45·45	−·01	−·05	45·39	48·51	−·01	−·08	48·42	52·10		−·10	52·00	51·25	59
14	46·30	−·01	−·07	46·22	45·91	−·01	−·07	45·83	49·33	·00	−·12	49·21	53·50		−·15	53·35	55·50	63
21	46·30	−·03	−·11	46·16	46·35	−·02	−·11	46·22	50·30	·00	−·20	50·10	53·40		−·12	53·28	58·25	71
28	46·30	−·02	−·08	46·20	46·85	−·02	−·09	46·74	50·70	·00	−·13	50·57	53·10		−·19	52·91	57·80	65
ly 5	46·30	−·03	−·07	46·20	47·30	−·02	−·07	47·21	51·08	·00	−·10	50·98	54·30		−·11	54·19	55·75	61
12	46·35	−·04	−·08	46·23	47·60	−·01	−·08	47·51	51·50	·00	−·14	51·36	53·45		−·19	53·26	55·25	65
19	46·40	−·04	−·08	46·28	48·01	−·01	−·09	47·91	51·60	·00	−·13	51·47	54·15		−·18	53·97	59·25	65
26	46·52	−·04	−·13	46·34	48·35	−·02	−·15	48·18	52·10	·00	−·24	51·86	54·62		−·32	54·30	61·50	75
ug. 2	46·60	−·05	−·12	46·43	48·62	−·01	−·14	48·47	52·41	·00	−·23	52·18	54·23		−·32	53·91	56·50	74
9	46·62	−·05	−·08	46·49	48·91	−·01	−·09	48·81	52·53	·00	−·13	52·40	55·00		−·17	54·83	57·50	65
16	46·75	−·05	−·11	46·59	49·20	−·01	−·14	49·05	52·78	·00	−·21	52·57	54·20		−·30	53·90	59·25	73
23	46·80	−·05	−·08	46·67	49·35	−·01	−·09	49·25	52·80	·00	−·13	52·67	55·00		−·17	54·83	59·00	65
30	46·92	−·05	−·09	46·78	49·60	−·01	−·09	49·50	52·00	·00	−·24	51·76	55·30		−·17	55·13	58·75	65
pt. 6	47·22	−·05	−·11	47·06	49·95	−·01	−·13	49·81	53·00	·00	−·18	52·82	53·21		−·26	52·95	58·50	70
13	47·16	−·04	−·13	46·99	50·03	−·01	−·16	49·96	52·09	·00	−·23	51·86	53·09		−·33	52·76	62·00	74
20	47·22	−·05	−·07	47·10	50·09	−·01	−·08	50·00	53·00	·00	−·10	52·90	54·90		−·11	54·79	54·50	62
27	47·35	−·05	−·08	47·22	50·30	−·01	−·09	50·20	53·00	·00	−·11	52·89	53·48		−·15	53·33	52·25	64
ct. 4	47·35	−·05	−·04	47·26	50·32	−·01	−·03	50·28	52·73	·00	−·02	52·71	52·63		−·03	52·60	49·25	55
11	47·49	−·04	−·07	47·38	50·40	·00	−·07	50·33	52·12	·00	−·08	52·04	51·30		−·13	51·17	48·75	60
18	47·50	−·03	−·01	47·46	50·31	·00	·00	50·31	51·50	·00	·00	51·50	49·50		−·03	49·47	43·25	51
25	47·60	−·03	+·01	47·58	50·25	·00	+·03	50·28	50·30	+·01	+·04	50·35	46·80		+·02	46·82	41·25	45
ov. 1	47·70	−·02	+·01	47·69	50·10	+·01	+·03	50·14	49·00	·00	+·03	49·03	45·80		+·01	45·81	38·50	45
8	47·80	−·01	−·02	47·77	49·80	+·01	−·01	49·80	48·10	·00	−·03	48·07	45·32		−·08	45·24	46·25	52
15	47·80	−·01	+·03	47·82	49·50	+·01	+·05	49·44	47·65	·00	+·05	47·70	44·80		+·03	44·83	33·75	42
22	47·85	+·01	+·05	47·91	49·15	+·01	+·06	49·08	46·52	+·01	+·06	46·59	41·40		+·03	41·43	32·25	38
29	47·80	+·01	−·05	47·76	48·80	+·02	−·05	48·77	45·20	+·01	−·09	45·12	40·10		−·15	39·95	36·25	58
ec. 6	48·00	+·02	+·01	48·03	48·42	+·02	+·01	48·45	44·30	·00	·00	44·30	42·20		−·03	42·17	41·25	45
13	48·00	+·02	+·01	48·03	47·98	+·01	+·01	48·00	44·40	·00	·00	44·40	42·00		−·03	41·97	40·75	45
20	47·90	+·03	+·09	48·02	47·40	+·02	+·07	47·49	43·92	+·01	+·07	44·00	40·51		+·08	40·59	31·50	32
27	47·92	+·03	+·04	47·99	47·10	+·02	+·03	47·15	43·00	·00	+·02	43·02	38·88		·00	38·88	33.25	40
1842.																		
n. 3	47·90	+·04	+·07	48·01	46·50	+·02	+·05	46·57	42·30	·00	+·03	42·33	40·00		+·04	40·04	33·25	35
10	47·81	+·05	+·09	47·95	46·25	+·02	+·06	46·33	42·10	+·01	+·04	42·15	38·50		·04	38·54	30·50	32
17	47·80	+·05	+·04	47·89	45·90	+·02	+·03	45·95	41·50	+·01	·00	41·51	37·70		−·01	37·69	31·75	40
24	47·70	+·05	+·06	47·81	45·70	+·02	+·04	45·76	41·00	+·01	+.02	41·03	37·50		·00	37·50	31·50	36
31	47·70	+·06	·00	47·76	45·20	+·02	·00	45·22	40·40	·00	−·03	40·37	37·80		−·06	37·74	37·50	47
eb. 7	47·60	+·06	+·05	47·71	44·80	+·02	+·03	44·85	40·30	·00	+·01	40·31	38·10		·00	38·10	31·50	37
14	47·51	+·06	+·02	47·59	44·55	+·02	·00	44·57	40·30	·00	−·01	40·29	38·60		−·03	38·57	37·25	44
21	47·50	+·06	·00	47·56	44·30	+·01	−·01	44·30	40·50	·00	−·03	40·47	39·50		−·06	39·44	38·25	48
28	47·40	+·06	−·01	47·45	44·10	+·01	−·02	44·09	40·90	·00	−·04	40·86	39·00		−·08	38·92	37·75	50
ar. 7	47·20	+·06	+·01	47·27	43·90	+·01	·00	43·91	40·60	·00	−·01	40·59	39·70		−·04	39·66	36·25	45

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5	T.
1842.																		
Mar. 14	47·10	+·05	−·02	47·13	43·80	+·01	−·03	43·78	40·80	·00	−·04	40·76	39·70	·00	−·10	39·60	41·25	53
22	47·00	+·05	−·03	47·02	43·70	+·01	−·03	43·68	41·30	·00	−·05	41·25	40·60		−·11	40·49	36·25	54
28	46·90	+·05	−·02	46·93	43·75	+·01	−·02	43·74	41·42	·00	−·04	41·38	40·39		−·09	40·30	41·50	51
Apr. 4	46·80	+·05	−·06	46·79	43·63	+·01	−·05	43·59	41·60	·00	−·08	41·52	41·50		−·16	41·34	40·75	60
11	46·71	+·04	−·04	46·71	43·62	·00	−·03	43·60	42·00	·00	−·06	41·94	42·50		−·11	42·39	45·50	55
18	46·70	+·04	−·07	46·67	43·70	·00	−·05	43·65	42·62	·00	−·09	42·53	43·50		−·17	43·33	44·75	61
25	46·55	+·03	−·06	46·52	43·80	·00	−·04	43·76	43·50	·00	−·08	43·42	45·70		−·16	45·54	50·00	60
May 2	46·50	+·02	−·08	46·44	44·10	·00	−·06	44·04	44·70	·00	−·11	44·59	47·50		−·18	47·32	31·50	63
9	46·41	+·01	−·07	46·35	44·20	−·01	−·05	44·14	45·50	·00	−·09	45·41	48·00		−·15	47·85	47·00	60
16	46·35	+·01	−·10	46·26	44·60	−·01	−·07	44·52	46·14	·00	−·14	46·00	48·50		−·22	47·28	53·25	66
23	46·25	·00	−·08	46·18	45·00	−·01	−·06	44·93	47·00	·00	−·11	46·89	49·30		−·17	49·13	51·75	62
30	46·24	·00	−·09	46·15	45·34	−·01	−·07	45·26	47·50	·00	−·12	47·38	50·10		−·19	49·91	55·00	64
June 6	46·30	−·01	−·13	46·16	45·60	−·01	−·10	45·49	48·30	−·01	−·20	48·09	52·30		−·30	52·00	59·25	72
13	46·30	−·02	−·17	46·11	46·10	−·02	−·17	45·91	49·65	−·01	−·29	49·35	55·24		−·46	54·78	63·00	83
20	46·20	−·03	−·08	46·09	46·40	−·02	−·08	46·30	51·10	−·01	−·14	50·95	56·50		−·14	56·36	57·25	65
27	46·30	−·04	−·09	46·17	47·10	−·02	−·10	46·98	52·20	−·01	−·15	52·04	55·60		−·18	55·42	57·25	67
July 4	46·30	−·04	−·06	46·20	47·50	−·02	−·06	47·42	52·34	·00	−·08	52·26	55·10		−·08	55·02	55·75	68
11	46·30	−·06	−·06	46·18	48·10	−·02	−·07	48·01	52·50	·00	−·08	52·42	54·60		−·08	54·52	54·25	60
18	46·40	−·05	−·09	46·26	48·45	−·02	−·09	48·34	52·70	·00	−·13	52·57	55·80		−·16	55·64	59·50	65
25																		
Aug. 1	46·70	−·06	−·16	46·58	49·20	−·02	−·20	48·98	54·30	−·01	−·31	53·98	58·40		−·42	57·98	64·25	81
8	46·50	−·07	−·08	46·35	49·50	−·02	−·10	49·38	54·90	−·01	−·12	54·77	58·60		−·11	58·49	61·25	65
15	46·80	−·07	−·10	46·63	50·00	−·02	−·14	49 94	55·35	·00	−·19	55·16	58·60		−·22	58·38	64·25	71
22																		
29	47·10	−·07	−·14	46·99	50·80	−·02	−·16	50·62	55·80	·00	−·24	55·56	58·10		−·34	57·76	60·00	76
Sept. 5	41·10	−·07	−·04	46·99	50·90	−·02	−·03	50·85	55·70	·00	·00	55·70	57·60		+·04	57·64	54·75	56
12	47·30	−·07	−·14	47·09	51·26	−·01	−·17	51·08	55·70	·00	−·23	55·47	56·72		−·31	56·41	57·80	73
19	47·40	−·07	−·02	47·31	51·30	−·01	·00	51·29	55·30	·00	+·03	55·27	56·70		+·09	56·79	51·25	52
26	47·50	−·06	−·04	47·40	51·50	−·01	−·03	51·46	55·10	·00	−·01	55·09	55·00		−·02	54 98	50·25	56
Oct. 3	47·60	−·06	−·03	47·51	51·55	−·01	−·02	51·52	54·30	·00	−·01	54·29	53·10		−·03	53·07	49·25	55
10	47·80	−·05	−·05	47·70	51·65	·00	−·04	51·61	53·51	·00	−·04	53·47	52·10		−·09	52·01	50·00	58
17	47·87	−·04	−·04	47·79	51·55	·00	−·02	51·53	52·90	·00	−·03	52·87	51·70		−·04	51·66	50·02	55
24	47·96	−·04	+·01	47·93	51·40	·00	+·03	51·37	52·10	·00	+·06	52·16	48·90		+·04	48·94	40·25	46
31	48·10	−·02	+·01	48·09	51·22	+·01	+·05	51·16	50·80	+·01	+·06	50·87	46·50		+·01	46·51	42·25	45
Nov. 7	48·12	−·01	+·03	48·10	50·95	+·01	+·06	50·88	49·80	·00	+·06	49·86	47·00		+·05	47·05	40·50	43
14	48·25	−·01	+·01	48·25	50·65	+·01	+·04	50·60	49·10	·00	+·03	49·13	46·10		+·01	46·11	39·25	45
21	48·30	−·01	+·04	48·33	50·28	+·01	+·07	50·20	48.20	+·01	+·06	48·27	44·31		+·04	44·35	34·75	40

Craigleith.

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5	T.
1837.																		
Feb. 4	46·54	+·03	+·02	46·59	43·54	·00	·00	43·54	41·02	·00	·00	41·02	39·15	·00	−·01	39·14		42
12	46·30	+·02	+·02	46·34	42·90	·00	+·01	42·91	41·42		·00	41·42	40·69		·00	40·69		40
21	46·02	+·02	+·01	46·05	42·88	·00	·00	42·88	41·52		·00	41·52	40·18		−·01	40·17		42
27	45·87	+·03	+·01	45·91	42·76	·00	·00	42·76	40·47		−·01	40·46	38·21		− 02	38·19		42
Mar. 6	45·69	+·02	+·01	45·72	42·70	·00	·00	42·70	40·77		−·01	40·76	39·50		−·02	39·48		44
13	45·57	+·02	·00	45·59	42·63	·00	·00	42·63	40·93		−·01	40·92	39·10		−·03	39·07		45
20	45·38	+·03	+·01	45·42	42·15	·00	·00	42·15	39·70		·00	39·70	38·00		−·02	37·98		42
27	45·22	+·03	+·02	45·27	41·98	·00	·00	41·98	39·71		·00	39·71	37·70		·00	37·70		38
Apr. 4	45·02	+·03	+·02	45·07	41·84	·00	·00	41·84	39·52		·00	39·52	37·86		·00	37·86		38
10	44·92	+·03	−·01	44·94	41·74	·00	−·01	41·73	39·77		−·02	39·75	38·73		−·04	38·69		48
17	44·71	+·02	+·01	44·74	41·73	·00	·00	41·73	40·42		−·01	40·41	39·68		−·01	39·67		43
24	44·60	+·02	−·01	44·61	41·68	·00	−·01	41·67	41·38		−·03	41·35	41·25		−·04	41·21		49
May 1	44·50	+·01	−·03	44·48	42·10	·00	−·02	42·08	42·52		−·05	42·47	43·15		−·09	43·06		57
8	44·37	+·01	·00	44·38	42·62	·00	·00	42·62	43·60		·00	43·60	44·45		−·01	44·44		45

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor, for Air.	t_4 corrected.	t_5.	T.
1837.	°			°	°			°	°			°	°			°	°	°
May 15	44·36	+·01	−·03	44·34	43·10	·00	−·02	43·08	44·00	·00	−·06	43·94	44·39	·00	−·09	44·30		57
22	44·38	·00	−·03	44·35	43·57	·00	−·02	43·55	45·28		−·07	45·21	46·58		−·09	46·49		58
30	44·44	−·01	−·04	44·39	44·19	·00	−·04	44·15	46·18		−·10	46·08	47·68		−·14	47·54		64
June 5	44·53	−·01	−·04	44·48	44·66	·00	−·04	44·62	47·10		−·11	46·99	49·05		−·15	48·90		64
12	44·67	−·01	−·06	44·60	45·28	·00	−·07	45·21	47·87		−·14	47·73	49·68		−·19	49·49		69
19	44·88	−·02	−·07	44·79	46·25	−·01	−·08	46·16	49·90		−·16	49·74	52·40		−·21	52·19		70
27	45·04	−·02	−·06	44·96	46·84	−·01	−·07	46·76	51·00		−·12	50·88	54·00		−·14	53·86		65
July 3	45·22	−·03	−·07	45·12	47·45	−·01	−·10	47·34	51·50		−·18	51·32	53·98		−·23	53·75		72
10	45·50	−·03	−·08	45·39	48·12	−·01	−·12	47·99	52·10		−·20	51·90	54·60		−·25	54·35		74
17	45·77	−·03	−·08	45·66	48·82	−·01	−·10	48·71	52·81		−·15	52·66	Above Scale.					69
24	46·10	−·03	−·08	45·99	49·68	−·01	−·12	49·55	Above Scale.									71
31	46·38	−·03	−·05	46·30	50·17	−·01	−·06	50·10										60
Aug. 7	46·72	−·03	−·09	46·60	50·62	−·01	−·11	50·50										71
14	46·94	−·03	−·09	46·82	50·47	−·01	−·11	50·35										70
21	47·15	−·03	−·07	47·05	50·66	·00	−·08	50·58										65
28	47·35	−·03	−·04	47·28	51·00	·00	−·04	50·90					54·60		−·04	54·56		58
Sept. 4	47·58	−·03	−·04	47·51	51·00	·00	−·05	50·95					53·40		−·08	53·32		60
12	47·84	−·02	−·07	47·75	51·00	·00	−·08	50·92	52·83		−·11	52·72	53·50		−·13	53·37		65
19	48·02	−·02	−·04	47·96	51·00	·00	−·04	50·96	52·30		−·05	52·25	52·45		−·06	52·39		57
25	48·12	−·02	−·04	48·06	50·70	·00	−·05	50·65	52·37		−·07	52·30	52·62		−·07	52·55		59
Oct. 2	48·25	−·02	−·06	48·17	50·60	·00	−·06	50·54	51·72		−·09	51·63	51·46		−·12	51·34		62
10	48·34	−·01	−·05	48·28	50·45	·00	−·06	50·39	51·85		−·07	51·78	52·10		−·09	52·01		60
17	48·39	−·01	−·04	48·34	50·37	·00	−·04	50·33	51·18		−·06	51·12	50·69		−·08	50·61		58
23	48·42	−·01	−·01	48·40	50·16	·00	·00	50·16	50·66		·00	50·66	49·95		·00	49·95		50
30	48·45	−·01	·00	48·44	49·83	·00	+·01	49·84	49·30		+·01	49·31	47·08		−·01	47·07		48
Nov. 6	48·47	·00	+·02	48·49	49·22	·00	+·03	49·25	47·50		+·03	47·53	46·70		+·03	46·73		43
13	48·40	·00	+·05	48·45	48·54	·00	+·06	48·60	47·27		+·07	47·34	45·35		+·07	45·42		36
20	48·40	+·01	+·03	48·44	47·90	·00	+·03	47·93	45·70		+·03	45·73	43·40		+·01	43·41		41
27	48·27	+·01	+·04	48·32	47·20	·00	+·03	47·23	45·10		+·03	45·13	42·95		+·03	42·98		39
Dec. 4	48·37	+·01	+·05	48·43	46·84	·00	+·04	46·88	44·43		+·03	44·46	42·55		+·04	42·59		37
11	48·00	+·02	+·05	48·07	46·10	·00	+·04	46·14	43·60		+·04	43·64	41·20		+·03	41·23		36
18	47·17	+·01	−·01	47·17	45·70	·00	−·01	45·69	43·18		−·03	43·15	41·70		−·05	41·65		50
25	47·61	+·03	−·01	47·63	45·21	·00	−·01	45·20	43·57		−·03	43·54	42·90		−·05	42·85		50
1838.																		
Jan. 2	47·33	+·01	+·02	47·36	45·24	·00	+·01	45·25	44·17		+·01	44·18	43·00		+·01	43·01		41
8	47·14	+·01	+·05	47·20	45·15	·00	+·03	45·18	43·60		+·04	43·64	41·48		+·04	41·52		34
15	46·92	+·02	+·07	47·01	44·87	·00	+·05	44·92	42·34		+·05	42·39	39·40		+·04	39·44		29
22	46·77	+·02	+·04	46·83	44·35	+·01	+·01	44·37	41·15		+·01	41·16	37·85		·00	37·85		38
29	46·65	+·03	+·04	46·72	43·42	+·01	+·01	43·44	40·00		+·01	40·01	37·27		+·01	37·28		35
Feb. 5	46·45	+·03	+·04	46·52	42·85	+·01	+ 01	42·87	39·62		+·01	39·63	37·00		·00	37·00		37
12	46·20	+·03	+·06	46·29	42·32	+·01	+·01	42·34	39·00		+·02	39·02	36·45		+·02	36·47		30
19	45·90	+·03	+·02	45·95	42·14	+·01	·00	42·15	38·50		·00	38·50	35·36		−·01	35·35		39
26	45·68	+·03	+·05	45·76	41·42	+·01	+·01	41·44	37·75		+·01	37·76	35·10		−·01	35·09		31
Mar. 5	45·45	+·04	+·02	45·51	40·80	+·01	·00	40·81	37·45		·00	37·45	35·50		−·01	35·49		39
12	45·20	+·03	+·01	45·24	40·70	·00	·00	40·70	38·08		−·01	38·07	37·15		−·02	37·13		43
19	44·92	+·03	−·01	44·94	40·07	·00	·00	40·07	39·20		−·02	39·18	38·17		−·03	38·14		47
26	44·65	+·03	·00	44·68	40·85	·00	·00	40·85	39·37		−·01	39·36	38·38		−·03	38·35		45
Apr. 2	44·40	+·02	−·01	44·41	41·20	·00	−·01	41·19	40·50		−·03	40·47	39·80		−·05	39·75		50
9	44·30	+·02	·00	44·32	41·34	·00	·00	41·34	41·02		−·01	41·01	40·58		−·02	40·56		45
16	44·20	+·01	+·01	44·22	41·63	·00	·00	41·63	41·68		+·01	41·69	41·88		+·01	41·89		40
23	44·07	+·01	·00	44·08	41·80	·00	·00	41·80	41·38		−·01	41·37	40·90		−·02	40·88		44
30	44·02	+·01	·00	44·03	42·05	·00	·00	42·05	42·10		−·01	42·09	42·05		−·02	42·03	39·0	45
May 8	44·04	+·01	−·06	43·99	42·38	·00	−·04	42·34	43·35		−·13	43·22	45·00?		−·22	44·78	42·3	73
14	44·00	·00	−·01	43·99	42·85	·00	−·01	42·84	44·58		−·01	44·57	45·62		−·01	45·61	43·2	47
21	44·03	·00	−·02	44·01	43·38	·00	−·01	43·37	44·45		−·04	44·41	45·20		−·06	45·14	46·0	52
28	44·17	·00	−·02	44·15	43·85	·00	−·01	43·84	45·15		−·04	45·11	46·58		−·05	46·53	47·4	52
June 4	44·23	·00	−·03	44·20	44·22	·00	−·03	44·19	45·80		−·06	45·74	47·25		−·10	47·15	49·2	57
11	44·32	−·01	−·02	44·29	44·68	·00	−·02	44·66	46·85		−·04	46·81	48·73		−·05	48·68	50·0	54
18	44·54	−·01	−·01	44·52	45·44	·00	−·02	45·42	47·90		−·03	47·87	50·10		−·02	50·08	51·3	52
25	44·70	−·01	−·04	44·65	46·03	·00	−·06	45·97	48·80		−·10	48·70	51·10		−·15	50·95	54·2	63
July 2	44·88	−·02	−·02	44·84	46·65	−·01	−·02	46·62	49·70		−·03	49·67	51·77		−·02	51·75	52·2	54
9	45·08	−·02	−·07	44·99	47·15	−·01	−·09	47·05	50·40		−·17	50·23	53·08		−·23	52·85	57·2	70
16	45·27	−·03	−·05	45·19	47·65	−·01	−·05	47·59	51·20		−·10	51·10	53·75		−·11	53·64	56·4	62
23	45·47	−·03	−·03	45·41	48·14	−·01	−·04	48·09	51·33		−·05	51·28	53·17		−·05	53·12	54·4	57

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5	T.
1838.	°			°	°				°			°	°			°	°	
July 30	45·74	−·03	−·05	45·66	48·54	−·01	−·07	48·46	51·32	·00	−·10	51·22	53·02	·00	−·13	52·89	56·1	63
Aug. 6	46·00	−·03	−·06	45·91	48·82	−·01	−·08	48·73	51·74		−·12	51·62	54·00		−·15	53·85	56·0	58
13	46·25	−·03	−·06	46·16	49·17	−·01	−·07	49·09	52·15		−·10	52·05	54·22		−·13	54·09	55·2	63
20	46·45	−·03	−·05	46·37	49·45	·00	−·06	49·39	52·05		−·07	51·98	53·62		−·08	53·54	55·2	60
27	46·73	−·03	−·08	46·62	49·68	·00	−·10	49·58	52·07		−·15	51·92	53·52		−·20	53·32	56·3	69
Sept. 3	46·90	−·03	−·06	46·81	49·80	·00	−·08	49·72	52·08		−·11	51·97	53·25		−·15	53·10	53·2	64
10	47·26	−·02	−·04	47·20	50·19	·00	−·04	50·15	51·75		−·04	51·71	52·00		−·06	51·94	48·4	57
17	47·28	−·02	−·04	47·22	49·85	·00	−·04	49·81	51·45		−·05	51·40	52·80		−·05	52·75	53·2	57
24	47·43	−·02	−·05	47·36	49·89	·00	−·05	49·84	51·35		−·08	51·27	51·88		−·10	51·78	51·1	60
Oct. 1	47·55	−·02	−·04	47·49	49·84	·00	−·04	49·80	51·10		−·06	51·04	51·90		−·07	51·83	52·2	58
8	47·62	−·01	·00	47·61	49·72	·00	+·01	49·73	50·20		+·01	50·21	49·68		+·01	49·69	47·1	49
15	47·72	−·01	−·02	47·69	49·45	·00	−·03	49·42	49·25		−·04	49·21	47·80		−·06	47·74	46·0	54
22	47·80	−·01	−·04	47·75	49·07	·00	−·04	49·03	48·60		−·06	48·54	48·16		−·09	48·07	49·2	57
29	47·78	−·01	+·01	47·78	48·76	·00	+·01	48·77	48·64		+·02	48·66	47·90		+·02	47·92	44·0	46
Nov. 5	47·85	·00	·00	47·85	48·48	·00	·00	48·48	47·20		·00	47·20	45·68		−·01	45·67	43·3	47
12	47·82	+·01	+·05	47·88	47·92	·00	+·05	47·97	46·32		+·06	46·38	44·70		+·08	44·78	37·3	36
19	47·74	+·01	+·04	47·79	47·43	·00	+·03	47·46	45·40		+·03	45·43	43·08		+·02	43·10	37·3	40
26	47·62	+·01	+·01	47·64	46·50	+·01	+·01	46·52	43·72		·00	43·72	41·42		−·03	41·39	40·2	45
Dec. 3	47·57	+·01	·00	47·58	46·12	·00	−·01	46·11	43·78		−·02	43·76	43·02		−·04	42·98	42·2	48
10	47·35	+·01	·00	47·36	45·85	·00	−·01	45·84	43·83		−·02	43·81	42·40		−·04	42·36	41·3	48
17	47·14	+·01	+·03	47·18	45·55	·00	+·02	45·57	43·40		+·01	43·41	41·68		+·01	51·69	38·3	40
27	46·95	+·02	+·03	47·00	44·85	·00	+·02	44·87	42·38		+·01	42·39	40·50		+·01	40·51	36·2	38
31	46·84	+·02	+·03	46·89	44·64	·00	+·01	44·65	22·12		+·01	42·13	40·85		+·01	40·86	36·3	39
1839.																		
Jan. 7	46·68	+·02	+·02	46·72	44·21	·00	+·01	44·22	41·77		+·01	41·78	40·00		·00	40·00	36·2	40
14	46·48	+·02	+·02	46·52	43·70	·00	+·01	43·71	40·95		·00	40·95	39·90		·00	39·90	37·4	40
21	46·28	+·03	+·02	46·33	43·40	+·01	+·01	43·42	40·25		·00	40·25	38·25		·00	38·25	34·3	39
28	46·06	+·03	+·03	46·12	43·07	+·01	+·01	43·09	40·04		+·01	40·05	38·02		+·01	38·03	33·0	36
Feb. 4	45·85	+·03	+·01	45·89	42·58	+·01	·00	42·59	39·28		·00	39·28	37·24		−·02	37·22	33·4	41
11	45·67	+·03	·00	45·70	42·12	·00	−·01	42·11	39·70		−·02	39·68	39·12		−·04	39·08	39·0	46
18	45·43	+·03	+·03	45·49	42·07	·00	+·01	42·08	40·10		+·01	40·11	38·90		+·02	38·92	34·0	35
25	45·23	+·03	+·01	45·26	41·95	·00	·00	41·95	39·33		−·01	39·32	37·98		−·02	37·96	35·0	42
Mar. 4	45·00	+·03	+·01	45·04	41·84	·00	·00	41·84	39·67		−·01	39·66	38·78		−·02	38·76	37·1	43
11	44·80	+·03	+·01	44·84	41·75	·00	·00	41·75	39·30		−·01	39·29	37·42		−·02	37·40	34·3	42
19	44·70	+·03	−·01	44·72	41·60	·00	·00	41·60	39·45		−·02	39·43	38·57		−·04	38·53	36·2	47
25	44·55	+·02	+·01	44·58	41·37	·00	·00	41·37	39·75		·00	39·75	39·43		−·02	39·41	36·4	42
Apr. 1	44·37	+·02	+·01	44·40	41·45	·00	·00	41·45	40·08		·00	40·08	39·22		−·01	39·21	36·3	40
8	44·22	+·02	−·01	44·23	41·55	·00	·00	41·55	39·68		−·02	39·66	38·65		−·05	38·60	47·2	49
15	44·10	+·02	−·01	44·11	41·45	·00	·00	41·45	40·26		−·02	40·24	40·98		−·05	40·93	41·3	48
22	44·00	+·01	−·02	43·99	41·68	·00	−·01	41·67	41·21		−·04	41·17	41·70		−·08	41·62	43·4	53
29	43·92	+·01	−·03	43·90	41·98	·00	−·02	41·96	42·32		−·06	42·26	43·41		−·11	43·30	46·2	57
May 6	43·90	·00	−·03	43·87	42·74	·00	−·02	42·72	43·40		−·06	43·34	43·80		−·10	43·70	45·2	56
13	43·88	·00	−·01	43·87	43·10	·00	−·01	43·09	43·90		−·03	43·87	44·06		−·13	43·93	44·0	50
21	43·93	·00	−·03	43·90	43·34	·00	−·03	43·31	44·15		−·07	44·08	45·50		−·12	45·38	47·3	58
27	43·98	·00	−·04	43·94	43·62	·00	−·04	43·58	44·80		−·09	44·71	46·10		−·16	45·94	48·4	63
June 3	44·07	−·01	−·02	44·04	44·04	·00	−·02	44·02	45·77		−·06	45·71	47·17		−·08	47·09	46·4	55
10	44·17	−·01	−·03	44·13	44·52	·00	−·03	44·49	46·58		−·07	46·51	48·60		−·10	48·50	49·4	58
17	44·31	−·01	−·06	44·24	45·05	·00	−·07	44·98	47·38		−·15	47·23	49·82		−·23	49·59	55·3	70
24	44·53	−·01	−·05	44·47	45·84	−·01	−·07	45·76	49·04		−·13	48·91	51·35		−·18	51·17	54·2	66
July 1	44·68	−·02	−·06	44·60	46·40	−·01	−·08	46·31	49·60		−·15	49·45	51·75		−·21	51·54	54·0	69
7	44·82	−·02	−·06	44·74	46·85	−·01	−·08	46·76	50·49		−·15	50·34	53·50		−·19	53·31	57·0	68
15	45·20	−·03	−·04	45·13	47·87	−·01	−·05	47·81	51·42		−·08	51·34	53·72		−·08	53·64	54·3	60
22	45·33	−·03	−·05	45·25	48·10	−·01	−·07	48·02	51·49		−·11	51·38	53·84		−·12	53·72	56·4	63
29	45·58	−·03	−·06	45·49	48·57	−·01	−·09	48·47	52·10		−·14	51·96	54·68		−·17	54·51	57·0	67
Aug. 5	45·87	−·03	−·08	45·76	49·04	−·01	−·11	48·92	52·47		−·18	52·29	54·70		−·23	54·47	56·4	71
14	46·17	−·03	−·05	46·09	49·44	·00	−·06	49·38	52·50		−·08	52·42	54·15		−·08	54·07	53·3	60
19	46·35	−·03	−·03	46·29	49·58	·00	−·03	49·55	52·10		−·04	52·06	53·27		−·04	53·23	52·3	56
26	46·62	−·03	−·06	46·53	49·68	·00	−·07	49·61	51·92		−·10	51·82	53·43		−·13	53·30	54·4	63
Sept. 3	46·85	−·02	−·05	46·78	49·78	·00	−·06	49·72	52·03		−·08	51·95	53·27		−·11	53·16	53·3	61
10	47·07	−·02	−·06	46·99	49·90	·00	−·07	49·83	51·95		−·09	51·86	53·17		−·12	53·05	53·4	62
16	47·28	−·02	−·04	47·22	50·09	·00	−·04	50·05	51·67		−·04	51·63	52·30		−·06	52·24	52·1	57
23	47·37	−·02	−·06	47·29	49·94	·00	−·07	49·87	51·30		−·10	51·20	51·67		−·13	51·54	51·0	62
30	47·47	−·02	−·04	47·41	49·78	·00	−·04	49·74	50·88		−·06	50·82	51·17		−·09	51·08	50·0	58
Oct. 7	47·60	−·01	−·04	47·55	47·65	·00	−·03	49·62	50·05		−·06	49·99	49·17		−·09	49·08	48·2	57

tes.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.	t_5.	*T.*
839.																		
. 14	47·66	−·01	−·02	47·63	49·34	·00	−·02	49·32	49·62	·00	−·02	49·60	49·64	·00	−·03	49·61	49·0	53
21	47·62	−·01	−·03	47·58	49·15	·00	−·03	49·12	49·12		−·05	49·07	48·20		−·08	48·12	46·2	56
28	47·80	−·01	·00	47·79	48·88	·00	·00	48·88	48·77		·00	48·77	48·37		−·01	48·36	45·1	49
v. 4	47·77	·00	·00	47·77	48·59	·00	+·01	48·60	47·94		+·01	47·95	46·98		·00	46·98	45·1	47
11	47·80	·00	·00	47·80	48·27	·00	·00	48·27	47·47		−·01	47·46	46·98		−·01	46·97	44·3	48
18	47·77	·00	·00	47·77	47·97	·00	·00	47·97	47·20		−·01	47·19	46·65		−·01	46·64	44·2	48
25	47·68	·00	+·03	47·71	47·68	·00	+·03	47·71	46·55		+·04	46·59	45·30		+·04	45·34	41·3	40
c. 2	47·60	+·01	+·06	47·67	47·20	·00	+·05	47·25	45·16		+·06	45·22	43·30		+·07	43·37	36·2	34
9	47·47	+·01	+·05	74·53	46·66	·00	+·05	46·71	44·20		+·05	44·25	41·68		+·06	41·74	35·2	34
16	47·43	+·02	+·03	47·48	45·94	·00	+·02	45·96	43·48		+·01	43·49	42·26		+·01	42·27	39·3	40
23	47·28	+·02	·00	47·30	45·50	·00	·00	45·50	43·40		−·01	43·39	42·68		−·02	42·66	42·1	46
30	47·03	+·02	+·06	47·11	45·25	·00	+·04	45·29	43·00		+·05	43·05	40·50		+·06	40·56	33·1	32
840.																		
n. 6	46·90	+·02	+·05	46·97	44·63	·00	+·03	44·66	42·20		+·03	42·23	40·39		+·04	40·43	34·2	34
13	46·69	+·02	−·01	46·70	44·27	·00	−·01	44·26	41·63		−·03	41·60	40·73		−·06	40·67	41·0	50
20	46·53	+·02	+·02	46·57	43·96	·00	+·01	43·97	41·88		+·01	41·89	40·83		+·01	40·84	37·1	40
27	46·32	+·02	+·04	46·38	43·73	·00	+·02	43·75	41·60		+·02	41·62	40·20		+·03	40·23	34·4	35
b. 3	46·14	+·03	+·02	46·19	43·41	·00	+·01	43·42	40·62		·00	40·62	39·00		−·01	38·99	36·3	40
10	45·97	+·03	·00	46·00	43·03	·00	·00	43·03	40·62		−·01	40·61	39·40		−·03	39·37	38·0	45
17	45·73	+·02	+·01	45·76	42·94	·00	·00	42·94	41·00		−·01	40·99	40·64		−·02	40·62	40·1	44
24	45·50	+·02	+·02	45·54	42·92	·00	+·01	42·93	40·80		·00	40·80	38·92		−·01	38·91	34·4	40
ar. 2	45·34	+·03	+·01	45·38	42·65	·00	·00	42·65	39·92		·00	39·92	37·82		−·01	37·81	34·0	41
9	45·20	+·03	−·03	45·20	42·29	·00	−·02	42·27	39·54		−·04	39·50	38·00		−·09	37·91	37·4	56
17	45·02	+·02	−·03	45·01	42·04	·00	−·02	42·02	40·44		−·04	40·40	40·46		−·10	40·36	40·4	56
24	44·80	+·02	·00	44·82	42·14	·00	·00	42·14	40·76		−·01	40·75	39·97		−·04	39·93	37·2	46
30	44·68	+·02	·00	44·70	42·16	·00	·00	42·16	41·00		−·02	40·98	41·28		−·03	41·25	42·0	46
pril 6	44·55	+·01	−·01	44·55	42·34	·00	−·01	42·33	41·70		−·02	41·68	41·95		−·05	41·90	42·3	49
14	44·45	+·01	−·02	44·44	42·55	·00	−·01	42·54	42·38		−·04	42·34	43·10		−·08	43·02	46·1	53
20	44·41	·00	−·04	44·37	42·82	·00	−·03	42·79	43·18		−·07	43·11	43·88		−·12	43·76	44·2	59
27	44·40	·00	−·05	44·35	43·22	·00	−·04	43·18	44·32		−·11	44·21	46·22		−·19	46·03	52·1	66
ay 4	44·38	−·01	−·02	44·35	43·76	·00	−·02	43·74	45·56		−·04	45·52	47·56		−·06	47·50	48·4	54
11	44·40	·00	·00	44·40	44·32	·00	·00	44·32	45·38		+·01	45·39	45·20		+·01	45·21	41·3	44
19	44·60	·00	−·02	44·58	44·60	·00	−·02	44·58	45·38		−·04	45·34	45·93		−·07	45·86	44·0	53
25	44·64	−·01	−·03	44·60	44·70	·00	−·03	44·67	45·89		−·05	45·84	47·38		−·08	47·30	48·3	55
ne 1	44·72	−·01	−·05	44·66	45·05	·00	−·05	45·00	46·65		−·10	46·55	48·68		−·15	48·53	53·3	63
8	44·84	−·01	−·05	44·78	45·57	·00	−·06	45·51	47·66		−·11	47·55	49·56		−·17	49·39	53·1	65
15	44·94	−·01	−·06	44·87	45·98	·00	−·06	45·92	48·48		−·12	48·36	50·50		−·18	50·32	52·4	65
22	45·18	−·02	−·06	45·10	46·49	·00	−·07	46·42	49·03		−·12	48·91	50·78		−·17	50·61	53·2	65
29	45·26	−·02	−·06	45·18	46·94	·00	−·08	46·86	49·42		−·14	49·28	51·76		−·19	51·57	55·1	67
ly 6	45·50	−·02	−·05	45·43	47·55	·00	−·05	47·50	50·26		−·08	50·18	52·10		−·10	52·00	54·4	60
13	45·63	−·02	−·05	45·56	47·84	·00	−·06	47·78	50·70		−·09	50·61	52·90		−·12	52·78	53·0	62
20	45·88	−·02	−·07	45·79	48·30	·00	−·10	48·20	51·32		−·16	51·16	53·68		−·21	53·45	56·2	69
27	46·05	−·03	−·07	45·95	48·68	·00	−·08	48·60	51·70		−·12	51·58	53·90		−·15	53·75	56·0	65
ug. 3	46·26	−·03	−·08	46·15	49·10	·00	−·10	49·00	52·20		−·17	52·03	54·39		−·21	54·18	58·0	69
10	46·47	−·03	−·07	46·37	49·45	−·01	−·09	49·35	52·76		−·12	52·64	55·58		−·15	55·43	59·0	66
18	46·78	−·03	−·07	46·68	50·09	·00	−·08	50·01	52·95		−·11	52·84	54·28		−·14	54·14	56·3	64
24	46·93	−·03	−·07	46·83	50·11	·00	−·09	50·02	52·97		−·13	52·84	54·81		−·16	54·65	54·3	66
31	47·15	−·03	−·07	47·05	50·34	·00	−·09	50·25	53·00		−·12	52·88	54·80		−·14	54·66	52·0	65
ept. 8	47·35	−·03	−·06	47·26	50·46	·00	−·07	50·39	52·70		−·09	52·61	53·72		−·11	53·61	50·1	62
14	47·50	−·02	−·05	47·43	50·44	·00	−·05	50·39	52·16		−·06	52·10	52·58		−·07	52·51	46·3	58
21	47·68	−·02	−·03	47·63	50·34	·00	−·03	50·31	52·30		−·03	52·27	51·36		−·05	51·31	47·0	55
28	47·83	−·02	−·07	47·74	50·14	·00	−·08	50·06	50·79		−·11	50·68	50·75		−·16	50·59	47·0	64
ct. 4	47·88	−·01	−·02	47·85	49·88	·00	−·02	49·86	50·13		−·02	50·11	49·56		−·03	49·53	42·0	53
12	47·95	−·01	−·01	47·93	49·55	·00	·00	49·55	49·42		−·01	49·41	48·92		−·01	48·91		50
19	48·00	−·01	−·02	47·97	49·23	·00	−·02	49·21	49·08		−·02	49·06	49·00		−·03	48·97		52
26	47·98	·00	·00	47·98	48·95	·00	·00	48·95	48·46		·00	48·46	46·98		−·01	46·97		48
ov. 2	48·02	·00	−·02	48·00	48·55	·00	−·01	48·54	47·50		−·03	47·47	46·60		−·05	46·55		52
9	47·92	·00	+·01	47·93	48·18	·00	+·01	48·19	47·08		+·01	47·09	45·70		+·01	45·71		45
16	47·94	+·01	·00	47·95	47·74	·00	·00	47·74	46·10		−·01	46·09	44·30		−·03	44·27		48
24	47·82	+·01	+·02	47·85	47·14	·00	+·01	47·15	44·78		+·01	44·79	43·01		−·01	43·00		44
30	47·70	+·01	−·02	47·69	46·84	·00	−·02	46·82	44·68		−·04	44·64	42·18		−·07	42·11		52
ec. 7	47·54	+·01	+·01	47·56	46·34	·00	·00	46·34	44·30		·00	44·30	43·25		−·02	43·23		45
14	47·34	+·01	+·05	47·40	46·00	·00	+·04	46·04	43·88		+·04	43·92	41·88		+·05	41·93		35
21	47·25	+·02	+·04	47·31	45·40	·00	+·03	45·43	42·76		+·03	42·79	40·90		+·03	40·93		36

Dates.	t_1 uncorrected.	Cor. for Col.	Cor. for Air.	t_1 corrected.	t_2 uncorrected.	Cor. for Col.	Cor. for Air.	t_2 corrected.	t_3 uncorrected.	Cor. for Col.	Cor. for Air.	t_3 corrected.	t_4 uncorrected.	Cor. for Col.	Cor. for Air.	t_4 corrected.
1840.																
Dec. 28	47·02	+·02	+·05	47·09	45·04	+·01	+·03	45·08	41·70	·00	+·03	41·73	38·98	·00	+·03	39·01
1841.																
Jan. 4	46·87	+·03	+·05	46·95	44·30	·00	+·02	44·32	41·15	·00	+·02	41·17	39·40		+·02	39·42
12	46·58	+·03	+·05	46·66	43·84	+·01	+·02	43·87	40·15	·00	+·02	40·17	37·17		+·01	37·18
18	46·44	+·03	+·05	46·52	43·12	+·01	+·02	43·15	39·38	·00	+·02	39·40	36·78		+·02	36·80
25	41·15	+·03	+·04	46·22	42·75	+·01	+·02	42·78	38·98	·00	+·01	38·99	36·50		+·01	36·51
Feb. 1	45·90	+·03	+·04	45·97	42·32	·00	+·01	42·33	39·28	·00	+·01	39·29	38·28		+·02	38·30
8	45·60	+·03	+·05	45·68	42·13	·00	+·01	42·14	39·13	·00	+·02	39·15	37·03		+·03	37·06
15	45·45	+·03	+·01	45·49	41·68	·00	·00	41·68	38·92	·00	−·01	38·91	38·40		−·03	38·37
22	45·22	+·03	−·01	45·24	41·64	·00	−·01	41·63	39·92	·00	−·02	39·90	39·45		−·06	39·39
Mar. 2	44·93	+·02	+·01	44·96	41·93	·00	·00	41·93	40·48	·00	−·01	40·47	39·60		−·01	39·57
8	44·80	+·02	−·03	44·79	42·02	·00	−·01	42·01	40·48	·00	−·04	40·44	40·28		−·09	40·19
15	44·66	+·01	−·02	44·65	42·14	·00	−·01	42·13	41·74	·00	−·04	41·70	42·58		−·09	42·49
22	44·54	+·01	−·02	44·53	42·54	·00	−·01	42·53	42·70	·00	−·03	42·67	43·20		−·06	43·14
29	44·48	+·01	−·03	44·46	42·94	·00	−·02	42·92	43·31	·00	−·06	43·25	43·97		−·10	43·87
Apr. 5	44·45	+·01	−·01	44·45	43·25	·00	−·01	43·24	43·38	·00	−·02	43·36	43·48		−·04	43·44
12	44·45	+·01	−·01	44·45	43·44	·00	−·01	43·43	43·48	·00	−·02	43·46	43·54		−·03	43·51
19	44·50	+·01	−·02	44·49	43·62	·00	−·02	43·60	43·85	·00	−·04	43·81	44·20		−·08	44·12
26	44·53	+·01	−·01	44·53	43·83	·00	−·01	43·82	44·10	·00	−·03	44·07	44·40		−·06	44·34
May 3	44·56	·00	−·01	44·55	44·08	·00	−·01	44·07	45·26	·00	−·03	45·23	46·78		−·02	46·72
10	44·64	·00	−·04	44·60	44·54	·00	−·04	44·50	45·84	·00	−·10	45·74	47·20		−·17	47·03
17	44·70	−·01	−·03	44·66	45·02	·00	−·04	44·98	47·09	·00	−·08	47·01	48·95		−·11	48·84
24	44·84	−·01	−·06	44·77	45·62	·00	−·08	45·54	47·80	·00	−·17	47·63	49·58		−·26	49·32
31	44·97	−·01	−·06	44·90	46·14	·00	−·07	46·07	48·29	−·01	−·13	48·15	51·42		−·20	51·22
June 7	45·12	−·02	−·03	45·07	46·73	·00	−·04	46·69	49·50	·00	−·07	49·43	51·19		−·08	51·11
14	45·30	−·02	−·04	45·24	47·17	−·01	−·05	47·11	50·05	·00	−·09	49·96	52·48		−·11	52·37
21	45·52	−·02	−·06	45·44	47·62	·00	−·06	47·56	50·19	·00	−·12	50·07	51·94		−·16	51·78
28	45·72	−·02	−·04	45·66	47·90	·00	−·06	47·84	50·28	·00	−·09	50·19	52·00		−·12	51·88
July 6	45·94	−·02	−·05	45·87	48·19	·00	−·06	48·13	51·02	·00	−·09	50·93	53·17		−·12	53·05
12	46·10	−·02	−·06	46·02	48·50	·00	−·07	48·43	50·97	·00	−·11	50·86	52·46		−·15	52·31
19	46·31	−·02	−·05	46·24	48·72	·00	−·07	48·65	51·18	·00	−·10	51·08	53·12		−·13	52·99
27	46·50	−·02	−·05	46·43	48·95	·00	−·05	48·90	51·40	·00	−·08	51·32	53·48		−·10	53·38
Aug. 2	46·66	−·02	−·08	46·56	49·18	·00	−·10	49·08	51·39	·00	−·17	51·22	52·64		−·22	52·42
9	46·80	−·02	−·04	46·74	49·27	·00	−·05	49·22	51·46	·00	−·08	51·38	53·10		−·09	53·01
16	47·00	−·02	−·06	46·92	49·45	·00	−·07	49·38	51·30	·00	−·10	51·20	52·43		−·14	52·29
23	47·20	−·02	−·04	47·14	49·71	·00	−·05	49·66	51·76	·00	−·07	51·69	52·98		−·09	52·89
30	47·27	−·02	−·06	47·19	49·68	·00	−·08	49·60	51·86	·00	−·12	51·74	53·58		−·16	53·42
Sept. 6	47·36	−·01	−·06	47·29	49·83	·00	−·02	49·81	51·60	·00	−·02	51·58	51·98		−·04	51·94
13	47·55	−·02	−·08	47·45	49·88	·00	−·10	49·78	51·40	·00	−·12	51·28	52·94		−·21	52·73
20	47·66	−·02	−·06	47·58	49·98	·00	−·06	49·92	52·25	·00	−·09	52·16	53·82		−·11	53·71
27	47·80	−·02	−·02	47·76	50·28	·00	−·02	50·26	52·00	·00	−·02	51·98	52·68		−·01	52·67
Oct. 4	47·88	−·02	−·02	47·84	50·25	·00	−·01	50·24	51·55	·00	·00	51·55	51·58		−·01	51·57
12	48·08	−·01	−·01	48·06	50·22	·00	−·01	50·21	50·58	·00	·00	50·58	50·48		−·02	50·46
18	48·15	−·01	+·02	48·16	49·95	·00	+·04	49·99	49·73	·00	+·05	49·78	48·86		+·06	48·92
26	48·15	·00	+·02	48·17	49·28	·00	+·04	49·32	47·96	·00	+·04	48·00	46·32		+·04	46·36
Nov. 1	48·15	·00	·00	48·15	48·75	·00	·00	48·75	46·96	·00	−·02	46·94	45·45		−·04	45·41
9	48·12	+·01	−·01	49·12	48·06	·00	−·01	48·05	46·22	·00	−·03	46·19	45·58		−·05	45·53
15	48·05	+·01	+·05	48·11	47·54	·00	+·05	47·59	45·88	·00	+·05	45·93	44·00		+·06	44·06
22	47·90	+·01	+·04	47·95	47·04	·00	+·03	47·07	43·95	·00	+·03	43·98	41·21		+·02	41·23
29	47·82	+·02	+·02	47·86	46·03	·00	+·01	46·04	42·70	·00	·00	42·70	40·78		−·01	40·77
Dec. 6	47·64	+·02	·00	47·66	45·36	·00	−·01	45·35	43·08	·00	−·03	43·05	42·43		−·05	42·38
13	47·38	+·02	+·02	47·42	45·10	·00	·00	45·10	43·08	·00	·00	43·08	42·02		·00	42·02
20	47·14	+·02	+·06	47·22	44·86	·00	+·03	44·89	42·30	·00	+·04	42·34	39·90		+·05	39·95
27	46·95	+·03	+·03	47·01	44·34	·00	+·01	44·35	41·24	·00	+·01	41·25	39·26		+·01	39·26
1842.																
Jan. 3	46·74	+·02	+·03	46·79	43·93	·00	+·01	43·94	41·61	·00	+·01	41·62	40·88		+·02	40·90
10	46·47	+·02	+·04	46·53	43·78	·00	+·02	43·80	40·85	·00	+·03	40·88	38·46		+·02	38·48
17	46·25	+·03	+·04	46·32	43·35	+·01	+·01	43·37	40·03	·00	+·01	40·04	37·68		+·02	37·68
25	46·07	+·03	+·02	46·12	42·60	·00	·00	42·60	39·57	·00	·00	39·57	37·72		·00	37·72
31	45·88	+·03	+·01	45·92	42·27	·00	·00	42·27	39·19	·00	·00	39·19	37·61		−·01	37·60
Feb. 7	45·58	+·03	+·04	45·65	42·19	·00	+·01	42·20	39·72	·00	+·02	39·74	38·17		+·03	38·20
15	45·40	+·03	·00	45·43	41·85	·00	·00	41·85	39·76	·00	−·01	39·75	38·98		−·04	38·94
21	45·17	+·02	+·02	45·21	41·90	·00	·00	41·90	40·22	·00	·00	40·22	39·96		+·01	39·97
Mar. 2	44·90	+·02	+·01	44·93	42·02	·00	·00	42·02	39·95	·00	·00	39·95	38·82		·00	38·82
7	44·88	+·02	−·01	44·89	41·78	·00	·00	41·78	39·95	·00	−·02	39·93	39·62		−·05	39·57